AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

552.
(Vol. XXVII.—October, 1892.)

HARDENING STRUCTURAL STEEL.

By A. C. Cunningham, Assoc. M. Am. Soc. C. E.
Read June 10th, 1892.

WITH DISCUSSION.*

After the idea of hardening steel eye-bars had occurred to the writer, inquiry developed the fact that the matter had been considered before; but as no definite information or data are available, the subject of hardening steel eye-bars, and steel generally, for structural purposes, will be considered independently.

Steel Eye-Bars.—The present practice in making steel eye-bars is to upset the bar, producing an enlargement on the end, which is then reheated and hammered to the shape of the head. The upsetting, hammering and partial heating of the bar, in forming the head, leaves the steel in an uncertain condition, but, without doubt, with internal stresses and more or less derangement of structure. To remove these

* See page 374.

internal stresses and allow the steel to adjust itself, the bar, after having the heads formed, is placed in a special furnace, brought to a red heat, and then allowed to cool slowly or anneal; the bar is now straightened and the pin holes drilled, which completes the manufacture.

Tables A and B are given in illustration of the derangement of structure due to upsetting, etc., and the re-adjustment after annealing; and it may be well to note here that the experience of the writer shows that the annealing of small specimens in lime is not always followed by a softening, or lowering of the ultimate strength of the specimens, but often by an increase of ultimate strength; there is nearly always, however, an increase of stretch and reduction, and an improvement of fracture.

The effect of the annealing in the furnace is to soften the bar, so that, when tested in full size, there is a loss of ultimate strength of from 2 000 to 6 000 pounds per square inch, as compared with the specimen test made on the material before annealing. This loss of strength is a fruitful source of annoyance to engineers and manufacturers, for if no allowance has been made in the specifications for the variation, steel may have been used which, in specimen test, before annealing, was near the lower limit, and when tested in the finished bar, is much too low in ultimate. On the other hand, if the manufacturer keeps the carbon up, so that the steel, in the specimen test, shall be near the upper limit of the specification, he may lose more or less of it on account of the steel testing beyond the upper limit.

If the operations in making steel eye-bars be carried on as above to the point where the steel reaches a red heat in the furnace, and if then, instead of being allowed to cool slowly, or anneal, the bars are removed and plunged in a tempering bath, the result will be a hardening or raising of the ultimate strength, proportional to the carbon in the steel, the temperature of the bar, and the temperature and nature of the bath.

The effects of hardening under various conditions are shown in Tables C, D and E.

The specimens which were quenched in oil were heated in a furnace especially adapted for this purpose, and were quite uniformly and evenly heated. The other specimens in Table C were heated in a rivet furnace, and those in Table D in a smith's forge fire, so that the conditions were rather against the tests. Attention is called to the excellent character of the fractures in all cases, and the low stretch and high

reduction indicate that a material is produced combining toughness and rigidity. The results of these specimen tests, necessarily made under unfavorable conditions, seem to indicate that with perfect control of the conditions, any desired results might be obtained.

Before adopting such a radical change in the manufacture of eye-bars, it would be advisable to thoroughly test a sufficient number in full size to show exactly what the effects would be, and the following method is suggested:

A plate to be rolled of such dimensions that it may be slotted into two eye-bar flats, and also give a sufficient number of specimen tests to show the condition of the steel at all points. The two flats to be made into eye-bars, one finished by the annealing method, and the other hardened, and both tested for comparison (see Sketch, page 357).

Compression Members.—Another, and fully as important, application of hardening steel, is in the compression members of structures. When it becomes necessary or desirable to use hard or high ultimate steel, the result is now reached by the addition of carbon, and a material is produced which must be worked and treated with great care; the holes must be drilled, and, if any piece is partially heated, the whole piece must be finally annealed. By using a soft steel, the ordinary shop practice may be followed and the steel finally hardened to the desired point, with the farther advantage that the heating will remove the internal stresses which may have been caused in punching and working.

The hardening property of steel has been taken advantage of in the case of springs, tools, weapons, etc., ever since they have been made of this material, and there seems to be no reason why it should not be used for structural material.

The writer hopes that the opinions and experience of those better versed on this subject than himself may be brought to bear on the question, and if there is any value in hardening structural steel, it may be taken advantage of.

TABLE A.

SPECIMEN TESTS CUT FROM THE HEAD OF A LARGE EYE-BAR AFTER THE SAME HAD BEEN UPSET AND HAMMERED TO SHAPE, AND BEFORE ANNEALING.

No.	Elastic Limit per square inch.	Ultimate Strength per square inch.	Per Cent. of Elongation in 8 Inches.	Per Cent. of Reduction of Area.	CHARACTER OF FRACTURE.
1	38 540	61 720	7.0	5.7	Square—100 per cent. granular. Broke in grip.
2	38 580	67 310	21.7	25.8	Square—100 per cent. granular.

TABLE B.

THESE TESTS ARE THE LONG ENDS OF TESTS IN TABLE A, ANNEALED IN LIME, AND RE-CUT IN PLANER.

No.	Elastic Limit per square inch.	Ultimate Strength per square inch.	Per Cent. of Elongation in 8 Inches.	Per Cent. of Reduction of Area.	CHARACTER OF FRACTURE.
1	38 220	67 600	20.3	47.8	Silky—angular.
2	38 420	66 700	19.0	49.2	Silky—angular.

TABLE C.

A BAR OF $\frac{5}{8}$-inch diameter Bessemer steel of very ordinary quality, as shown by the analysis, Carbon 0.10 per cent., Phos. 0.15 per cent., Mang. 0.60 per cent., was cut into nine pieces, which were numbered consecutively from one end. 1 and 9 were tested for uniformity of bar, 2, 4, 6 and 8 were quenched in water at various temperatures, and 3, 5 and 7 were quenched in fish oil.*

No.	Bath.		Test quenched from	Elastic limit per sq. in.	Ultimate per square inch.	Per cent. elong. in		Per cent. red. of area.	Character of fracture.	
	Nature.	Tem. Dg. F.				2-in.	8-in.			
1..	Not quenched		Cherry red.	40 350	65 200	42.0	25.0	59.5	Silky	½ cup.
9..	Not quenched		"	40 420	64 540	35.0	20.0	59.1	"	"
2..	Water....	59	"	43 680	87 030	25.0	13.7	43.5	"	"
4..	"	68	"	42 600	85 540	27.0	15.0	41.6	"	"
6..	"	77	"	42 050	80 840	28.0	14.2	44.7	"	"
8..	"	88	"	44 000	75 620	29.0	13.2	54.9	"	"
3..	Fish oil..	90	"	41 700	68 130	38.0	22.2	56.6	"	"
5..	" ..	90	"	41 400	67 800	40.0	22.7	57.0	"	"
7..	" ..	90	"	40 800	67 040	38.5	22.5	57.3	"	"

* See Plate L for view of fractures.

TABLE D.

A BAR OF ½-inch diameter open-hearth steel of good quality, as shown by the analysis, Carbon 0.20 per cent., Phos. 0.05 per cent., Mang. 0.50 per cent., was cut into twenty-five pieces, which were numbered consecutively from one end. Five pieces were tested for uniformity of bar, and the balance were quenched as indicated in the table.*

No.	Bath. Nature.	Bath. Tem. Dg. F.	Test quenched from	Elastic limit per sq. in	Ultimate per sq. in.	Per cent. elong. in 2-in.	Per cent. elong. in 8-in.	Per cent. red. of area.	Character of fracture.	Group No.
1	Not quenched	...	Cherry red.	44 180	60 300	42.0	26.5	59.6	Silky ¾ cup.	
7	Not quenched	...	"	42 980	59 040	41.0	26.0	57.1	" ½ "	
13	Not quenched	...	"	40 830	59 380	37.0	23.8	57.4	" ½ "	1
19	Not quenched	...	"	39 500	58 210	38.5	25.0	56.9	" .. "	
25	Not quenched	...	"	41 070	57 750	38.5	24.8	59.3	" ½ "	
2	Water....	56	"	45 930	79 690	25.0	18.2	42.9	" ½ "	
3	"	56	"	45 450	76 500	24.0	17.7	44.5	" .. "	
4	"	56	"	45 830	75 360	28.0	14.2	56.5	" ½ "	2
5	"	56	"	46 250	75 720	26.0	11.2	57.4	" ½ "	
6	"	56	"	41 250	76 380	26.0	15.7	45.3	" ¾ "	
8	Soap and water..	80	"	45 300	63 520	28.0	18.0	59.3	" .. "	
9	Soap and water..	80	"	46 250	63 500	35.0	22.5	54.2	" ¾ "	
10	Soap and water..	80	"	41 580	64 980	38.0	25.0	55.6	" ¾ "	3
11	Soap and water..	80	"	45 030	64 200	33.0	21.7	58.4	" .. "	
12	Soap and water..	80	"	41 220	64 330	34.0	19.5	55.6	" ½ "	
14	Boiling water..	212	"	45 370	63 410	35.0	23.7	56.0	" .. "	
15	Boiling water..	212	"	43 020	64 840	37.0	24.2	59.5	" ½ "	
16	Boiling water..	212	"	45 830	61 100	39.0	24.5	56.5	" .. "	4
17	Boiling water..	212	"	41 500	62 320	38.0	20.5	57.8	" .. "	
18	Boiling water..	212	"	45 040	65 750	32.0	23.0	57.6	" ¾ "	
20	Fish oil..	90	"	46 950	64 180	39.0	21.7	55.8	" ½ "	
21	" ..	90	"	45 560	63 140	40.0	22.0	54.4	" ½ "	
22	" ..	90	"	44 000	63 140	40.0	22.5	53.0	" .. "	5
23	" ..	90	"	44 000	63 140	37.0	20.7	54.4	" ½ "	
24	" ..	90	"	45 030	64 700	37.0	20.7	57.1	" ½ "	

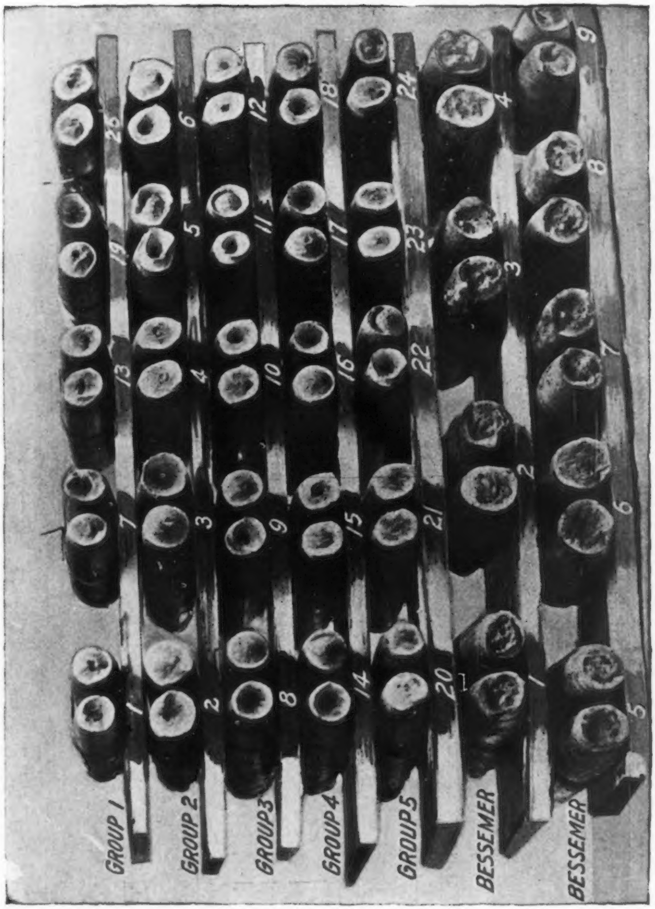
* See Plate L for view of fractures.

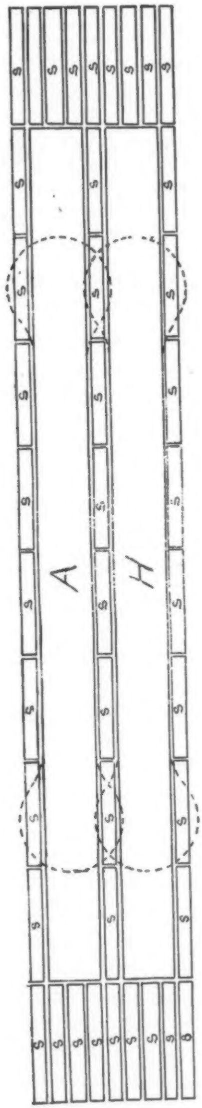
TABLE E.

AVERAGES of the Five Groups in Table D.

Group No.	Bath.		Test quenched from	Elastic limit per sq. in.	Ultimate per sq. in.	Per cent. elong. in		Per cent. red. of area.	Character of fracture.
	Nature.	Tem. Dg. F.				2-in.	8-in.		
1	Not quenched	...	Cherry red.	41 710	58 930	39.4	25.2	58.0	Silky, partial cup.
2	Water....	56	"	44 940	76 730	25.8	15.4	49.3	" "
3	Soap and water..	80	"	43 870	64 100	33.6	21.3	56.6	" "
4	Boiling water..	212	"	44 150	63 440	36.2	23.6	57.5	" "
5	Fish oil..	90	"	45 110	63 660	38.6	21.5	54.9	" "

PLATE L.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 552.
CUNNINGHAM ON HARDENING STEEL.




S
A
H

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

553.

(Vol. XXVII.—October, 1892.)

THE RESULTS OBTAINED FROM TESTS OF FULL-SIZED STEEL EYE-BARS.

By FREDERICK H. LEWIS, Esq.

READ JUNE 10TH, 1892.

WITH DISCUSSION.*

About two years ago the writer had occasion to note some rather large differences in ultimate strength in tests of full-sized steel eye-bars, as compared with the small specimen tests of the same material. Full-sized bars of moderate sections showed losses in ultimate strength of from 5 000 to 10 000 pounds per square inch, and quite notable decreases in elastic limit also. After investigating these results carefully until satisfied of the facts, the comparison was extended to quite a series of other tests, and the conclusion was reached that these losses were the rule and not the exception, and that they were frequently large. Here, for instance, in Table No. 1, is a comparison of results which show how the figures run.

* See page 374.

TABLE No. 1.

COMPARISON OF TESTS OF FULL-SIZED ANNEALED EYE-BARS WITH SMALL SPECIMEN TESTS CUT FROM MATERIAL ROLLED FROM THE SAME MELT OF STEEL.

SIZE.	AREA.	ULTIMATE STRENGTH.		
		Specimen.	Eye-Bar.	Loss on Eye-Bar.
5 x 13/16	4.06	63 880	63 500	380
5 x 7/8	4.44	67 140	63 190	3 950
4 x 1 1/8	4.48	68 400	66 010	2 390
5 x 7/8	4.53	68 180	65 280	2 900
6 x 7/8	5.26	68 200	58 170	10 030
4 x 1 5/16	5.27	64 660	58 990	5 670
5 x 1 1/16	5.48	63 100	63 640	+ 540
4 x 1 3/8	5.53	60 200	59 640	560
5 x 1 1/8	5.67	66 720	66 860	+ 140
5 x 1 1/8	5.85	65 500	58 300	7 200
5 x 1 3/16	5.86	67 915	58 820	9 095
6 x 1	6.05	65 500	63 920	1 580
6 x 1	6.15	64 060	60 420	3 640
6 x 1	6.16	63 220	64 000	+ 780
5 x 1 1/4	6.22	68 610	64 600	4 010
6 x 1 1/16	6.26	65 420	61 770	3 650
5 x 1 1/4	6.28	64 560	65 200	+ 640
6 x 1 1/16	6.53	65 720	64 660	1 060
5 x 1 5/16	6.58	64 420	62 230	2 190
5 x 1 3/8	6.79	65 070	57 870	7 200
5 x 1 3/8	6.80	63 540	60 510	3 030
5 x 1 3/8	6.83	65 180	61 900	3 280
5 x 1 3/8	6.93	64 210	56 920	7 290
5 x 1 3/8	6.96	62 940	62 640	300
5 x 1 3/8	6.97	67 230	59 030	8 200
7 x 1	7.01	65 030	56 060	8 970
7 x 1	7.03	62 220	57 160	5 360
5 x 1 7/16	7.08	63 450	58 120	5 330
5 x 1 7/16	7.14	65 080	64 780	380
6 x 1 3/16	7.15	63 300	60 980	2 320
6 x 1 1/4	7.51	62 090	63 100	+ 1 010
6 x 1 1/4	7.56	65 540	59 890	5 650
5 x 1 1/2	7.66	67 440	58 050	9 390
5 x 1 9/16	7.88	66 930	61 150	5 780
5 x 1 5/8	8.08	64 080	63 810	270
5 x 1 5/8	8.10	64 560	62 250	2 310
5 x 1 5/8	8.25	64 350	60 660	3 690
5 x 1 11/16	8.42	65 470	56 790	8 680
7 x 1 7/16	9.86	66 670	60 240	6 430
7 x 1 1/2	10.23	61 920	62 800	+ 880
7 x 1 5/8	11.51	64 270	63 080	1 190
7 x 1 13/16	12.83	59 940	59 930	10
7 x 1 13/16	12.90	61 500	59 290	2 210
8 x 2 7/16	19.46	67 130	56 890	10 240

The tests in this table were taken at random from a large file of eye-bar tests, the only principle of selection being to secure those whose small specimen tests could be most easily verified. The list is representative, including bars forged by several shops. The small specimen tests were cut from bars rolled from the same melts of steel as the eye-bars tested. Generally, but not always, the specimen tests were cut

from bars of the same sizes as the eye-bars; they were not, however, cut from the identical bars.

The writer failed to get much satisfaction from the figures excepting the general conclusion stated above. No law of variation was apparent, and yet definite information on the subject was clearly of much practical importance. In order, therefore, to get a little nearer to the subject, he finally concluded to get some comparative tests, in which the specimen tests should be cut from the identical bars which were afterward tested as eye-bars.

Such tests have accordingly been made under his direction, whenever circumstances rendered it practicable to get them, for the past two years; and as a result he now has a record of thirty-five good tests, covering a considerable range of sizes. By "good tests" is meant simply that the bars when tested gave every evidence of skillful forging and annealing. This series of tests is given in full in Table No. 2.

The tests are representative ones, including bars forged by the Edge Moor, Union, Pencoyd and Phœnix Bridge companies, and material rolled by the Cambria, Carnegie, Pencoyd and Phœnix Mills. The specimen tests were cut from the bars in their natural state as they came from the mills; the eye-bars were annealed after forging in the usual way. It should be noted that a number of these specimen tests were made as a special matter, quite independent of the regular tests on which the material was accepted at the mill.

This table gives the complete record of these tests, so that all the facts may be available; but for the purpose of readily getting at the gist of the matter, Table No. 3, giving a comparison of the results obtained, has been prepared.

From bars 2, 7, 11, 13, 14, 18, 23 and 24, sections were also cut off at the mills and annealed there, and specimen tests were cut from these annealed pieces. A comparative table of the results obtained from these tests is given in Table No. 4.

TABLE No. 2.

Results Obtained from the Identical Steel Bars in Small Specimen Tests and in Full-Sized Annealed Eye-Bars.

No.	Size of Bar.	Area.	Specimen Tests.				Eye-Bar Tests.			
			Elastic Limit.	Ultimate Strength.	Stretch in 8 inches	Reduction.	Elastic Limit.	Ultimate Strength.	Stretch in gaug'd length.	Reduction.
1	$4 \times \frac{3}{4}$	2.98	36 660	66 600	30.7	52.8	39 030	64 340	20.1	53.1
2	$3 \times 1\frac{1}{16}$	3.19	40 730	68 480	27.5	50.0	39 640	67 770	12.5	49.8
3	$5 \times \frac{3}{4}$	3.81	38 340	65 680	27.0	55.3	34 790	61 040	13.6	49.3
4	4×1	4.04	39 870	70 630	27.5	47.4	36 850	64 620	14.0	46.8
5	$5 \times \frac{13}{16}$	4.16	37 670	68 560	26.5	47.9	38 730	65 210	12.5	45.9
6	$5 \times \frac{7}{8}$	4.35	37 840	67 030	23.2	46.7	43 600(?)	66 110	10 9	51.5
7	$4 \times 1\frac{1}{8}$	4.47	41 670	68 750	23.7	48.7	38 780	66 610	14.3	46.7
8	5×1	5.02	39 870	63 480	26.2	52.0	39 820	62 980	15.9	51.2
9	5×1	5.07	39 030	59 810	29.5	59.3	36 640	58 330	13.8	49.7
10	$5 \times 1\frac{1}{8}$	5.62	33 570	62 780	25.5	47.7	35 930	63 400	15.2	42.5
11	$5 \times 1\frac{3}{16}$	5.90	39 650	70 080	22.5	52.1	37 330	68 100	12.3	44.7
12	6×1	6.11	37 370	60 080	31.0	62.5	38 480	58 330	17.4	52.7
13	$5 \times 1\frac{1}{4}$	5.25	40 650	69 650	22.0	44.5	36 880	65 260	11.4	41.7
14	$5 \times 1\frac{1}{4}$	6.25	40 570	70 000	25.2	44.5	38 180	66 250	12.0	48.0
15	$5 \times 1\frac{5}{16}$	6.53	36 980	72 840	21.5	49.2	36 540	64 660	19.0	46.1
16	$5 \times 1\frac{3}{8}$	6.91	37 030	61 390	24.5	49.2	38 080	62 000	16.3	48.0
17	7×1	7.01	35 240	68 850	27.5	47.2	35 520	61 200	15.7	55.9
18	$5 \times 1\frac{7}{16}$	7.29	39 280	67 740	25.5	47.4	37 170	64 630	11.0	50.1
19	$5 \times 1\frac{1}{2}$	7.52	37 220	62 740	31.5	59.3	35 790	58 990	14.4	51.6
20	$6 \times 1\frac{1}{4}$	7.56	38 690	64 500	21.2	54.2	35 880	59 820	17.5	51.3
21	$6 \times 1\frac{1}{4}$	7.61	36 530	64 210	27.5	47.5	34 840	61 110	15.0	46.0
22	$6 \times 1\frac{3}{8}$	8.29	32 310	64 520	25.7	46.4	34 440	58 800	18.0	51.7
23	$5 \times 1\frac{11}{16}$	8.30	42 100	71 580	23.7	48.8	38 830	68 800	13.0	40.4
24	$5 \times 1\frac{11}{16}$	8.35	37 950	66 770	26.0	45.1	35 170	63 020	12.6	52.9
25	$8 \times 1\frac{1}{16}$	8.46	38 690	63 770	26.0	52.7	40 330	61 840	13.1	46.0
26	$8 \times 1\frac{1}{16}$	8.48	36 450	59 100	31.0	62.5	37 550	58 120	15.7	46.3
27	$6 \times 1\frac{13}{32}$	8.62	36 530	64 840	27.0	49.7	32 420	58 680	14.2	47.9
28	$5 \times 1\frac{3}{4}$	8.82	33 560	62 780	25.5	47.7	34 690	63 130	19.1	44.3
29	$8 \times 1\frac{1}{4}$	10.05	34 590	58 600	33.0	64.3	34 710	56 210	17.8	49.1
30	$8 \times 1\frac{5}{16}$	10.36	39 400	72 400	23.5	42.0	36 960	62 210	14.3	47.7
31	$8 \times 1\frac{5}{16}$	10.44	38 590	66 820	27.5	48.6	37 040	62 460	15.1	48.3
32	$8 \times 1\frac{5}{16}$	10.53	36 090	59 160	31.0	59.9	36 720	57 610	19.7	48.9
33	$6 \times 1\frac{13}{16}$	10.74	37 490	64 830	26.7	50 8	36 080	60 770	18.1	50.7
34	8×2	15.96	41 740	65 450	28.0	50.4	31 310	53 670	15.0	51.8
35	8×2	16.02	40 400	68 550	25.0	44.7	35 640	60 730	15.0	49.1

TABLE No. 3.

Comparison of Results Given in Table No. 2, Showing Losses in Full-Sized Eye-Bar Tests—Identical Series.

No.	Size of Bar.	Area.	Elastic Limit.			Ultimate Strength.		
			Specimen Tests.	Eye-Bar Tests.	Loss in Eye-Bar Tests.	Specimen Tests.	Eye-Bar Tests.	Loss in Eye-Bar Tests.
1......	4 x $\frac{3}{4}$	2.98	36 660	39 030	+ 2 370	66 600	64 340	2 260
2......	3 x 1$\frac{1}{16}$	3.19	40 730	39 640	1 090	68 480	67 770	710
3......	5 x $\frac{3}{4}$	3.81	38 340	34 790	3 550	65 680	61 040	4 640
4......	4 x 1	4.04	39 870	36 850	3 020	70 650	64 620	6 030
5......	5 x $\frac{13}{16}$	4.16	37 690	38 730	+ 1 040	68 560	65 210	3 350
6......	5 x 1$\frac{7}{8}$	4.35	37 840	43 600(?)	+ 5 760	67 030	66 110	920
7......	4 x 1$\frac{1}{8}$	4.47	41 670	38 780	2 890	68 750	66 610	2 140
8......	5 x 1	5.02	39 870	39 820	50	63 480	62 980	500
9......	5 x 1	5.07	39 030	36 640	2 390	59 810	58 330	1 480
10......	5 x 1$\frac{1}{8}$	5.62	33 570	35 930	+ 2 360	62 780	63 400	+ 620
11......	5 x 1$\frac{3}{16}$	5.90	39 650	37 330	2 320	70 080	68 100	1 980
12......	6 x 1	6.11	37 370	38 480	+ 1 110	60 080	58 330	1 750
13......	5 x 1$\frac{1}{4}$	6.25	40 650	36 880	3 770	69 650	65 260	4 390
14......	5 x 1$\frac{1}{4}$	6.25	40 570	38 180	2 390	70 000	66 250	3 750
15......	5 x 1$\frac{5}{16}$	6.53	36 980	36 540	440	72 840	64 660	8 180
16......	5 x 1$\frac{3}{8}$	6.91	37 030	38 080	+ 1 050	61 390	62 000	+ 610
17......	7 x 1	7.01	35 240	35 520	+ 280	68 850	61 200	7 650
18......	5 x 1$\frac{7}{16}$	7.29	39 280	37 170	2 110	67 740	64 630	3 110
19......	5 x 1$\frac{1}{2}$	7.52	37 220	35 790	1 430	62 720	58 990	3 730
20......	6 x 1$\frac{1}{4}$	7.56	38 690	35 880	2 810	64 500	59 880	4 620
21......	6 x 1$\frac{1}{4}$	7.61	36 530	34 840	1 690	64 210	61 110	3 100
22......	6 x 1$\frac{3}{8}$	8.29	32 310	34 440	+ 2 130	64 520	58 800	5 720
23......	5 x 1$\frac{11}{16}$	8.30	42 100	38 830	3 270	71 580	68 800	2 780
24......	5 x 1$\frac{11}{16}$	8.35	37 950	35 170	2 780	66 770	63 020	3 750
25......	8 x 1$\frac{1}{16}$	8 46	38 690	40 330	+ 1 640	62 770	61 840	930
26......	8 x 1$\frac{1}{16}$	8.48	36 450	37 550	+ 1 100	59 100	58 120	980
27......	6 x 1$\frac{13}{32}$	8.62	36 530	32 420	4 110	64 840	58 680	6 160
28......	5 x 1$\frac{3}{4}$	8.82	33 560	34 690	+ 1 130	62 780	63 130	+ 350
29......	8 x 1$\frac{1}{4}$	10.05	34 590	34 710	+ 120	58 600	56 210	2 390
30......	8 x 1$\frac{5}{16}$	10.36	39 400	36 960	2 440	72 400	62 210	10 190
31......	8 x 1$\frac{5}{16}$	10.44	38 590	37 040	1 550	66 820	62 460	4 360
32......	8 x 1$\frac{5}{16}$	10.53	36 090	36 720	+ 630	59 160	57 610	1 550
33......	6 x 1$\frac{13}{16}$	10.74	37 490	36 080	1 410	64 830	60 770	4 060
34......	8 x 2	15.96	41 740	31 310	10 430	65 450	53 670	11 780
35......	8 x 2	16.02	40 400	35 640	4 760	68 550	60 730	7 820
Totals..................			1 330 370	1 290 390	....	2 302 030	2 176 870	....
Net total loss............			....	39 980	....	....	125 180	....

Or an average loss per bar of 1 142 pounds and 3 595 pounds respectively in elastic limit and ultimate strength.

TABLE No. 4.

COMPARISON OF RESULTS OBTAINED FROM ANNEALED SPECIMEN TESTS WITH RESULTS GIVEN IN TABLES NOS. 2 AND 3.

No.	Elastic Limit.						Ultimate Strength.					
	Specimen in natural state.		Annealed specimen.		Eye-bar.		Specimen in natural state.		Annealed specimen.		Eye-bar.	
	Pounds.	Per cent.	Pounds.	Per cent.	Pounds.	Per cent.	Pounds.	Per cent.	Pounds.	Per cent.	Pounds.	Per cent.
2	40 730	100.0	39 730	97.5	39 640	97.3	68 480	100.0	68 680	100.3	67 770	99.0
7	41 670	100.0	38 880	93.4	38 780	93.1	68 750	100.0	66 850	97.2	66 610	96.9
11	39 650	100 0	39 280	99.1	37 330	94 1	70 080	100.0	69 040	98.5	68 100	97.2
13	40 650	100.0	39 760	97.8	36 880	90.7	69 650	100.0	68 580	98.5	65 260	93.7
14	40 570	100.0	40 340	99.4	38 180	94.1	70 000	100.0	69 900	99.9	66 250	95.1
18	39 280	100.0	38 900	99.0	37 170	94.6	67 740	100.0	67 220	99.2	64 630	95.4
23	42 100	100.0	41 490	98.6	38 830	92.2	71 560	100.0	71 420	99.8	68 800	96.1
24	37 950	100.0	37 690	99.3	35 170	92.7	66 770	100.0	66 310	99.3	63 020	94.4

The whole series, as given in Tables Nos. 2, 3 and 4, constitute a unique and interesting record, and as it affords sufficient information (in connection with well-known facts we already have) to reach reasonably clear conclusions about these losses in full-sized steel bars, the results are now presented.

On the face of it, Table No. 3 is chiefly remarkable for its irregularities, especially in the matter of elastic limit (which will be more fully discussed later on). It is clearly more regular than Table No. 1, but, still, the fact that there is a large amount of accidental variation is apparent. It is impracticable to plat the results into a line or curve, either on the basis of area or of the area divided by the perimeter. It is impossible to predict from it that a bar of such a size will lose so much in full-sized test, or that such another bar will lose so much more or so much less than the first.

The fact is, regularity should not be expected. Steel is a wonderfully sensitive metal, reacting quickly under different conditions of work, temperature and chemical composition. There are, consequently, too many factors present in most cases to render a solution practicable.

On its face, Table No. 3 shows clearly but two things, viz.: 1st, that all steel bars lose in ultimate strength when tested in full-sized annealed eye-bars (the three tests which show an increase in ultimate

strength are disregarded in this conclusion, because the increases are so small that they are well within the limit of error in testing); and 2d, that, regardless of size, any bar may lose as much as 3 000 or 4 000 pounds in ultimate strength in full-sized test, and occasional bars will lose 10 000 to 12 000 pounds. So much is readily apparent.

But a little closer examination shows that under all the apparent irregularity there is, after all, a good deal of uniformity. Any one who examines the results given by the full-sized bars in Table No. 2 will probably be surprised to find that in the comparative figures given in Table No. 3 the losses in ultimate strength amount to such large figures in a number of tests. With, perhaps, one exception, every eye-bar test would be likely to secure the approval of bridge engineers. As a matter of fact, every one of these tests was acceptable to the engineer in whose interest it was made. The reason why this is true is found to lie in the fact that the large losses occur generally in bars which give high figures in specimen tests, while the smaller losses and gains occur in tests which show low figures in small specimen tests.

The specifications under which these bars were made varied a little, but for practical purposes they all required the manufacturers to make a steel with a mean ultimate strength of about 64 000 to 65 000 pounds per square inch, with 3 000 or 4 000 pounds leeway either above or below these figures. Now, out of the thirty-five tests there are twenty-four having ultimates in specimen tests exceeding 64 000 pounds, and these show average losses of 4 720 pounds each in full-sized tests; but there are eleven with ultimates under 64 000 pounds in specimen tests, and these show average losses of but 1 068 pounds each. The same thing is true of the series of tests in Table No. 1; the thirty-one tests having ultimates over 64 000 pounds in specimen tests show average losses of 4 368 pounds each; but the thirteen tests having ultimates under 64 000 pounds in specimen tests, show losses of but 1 215 pounds. That this is not accidental is shown by the details. The losses increase with some regularity as the specimen tests give higher figures. Thus, in the identical series, bars testing below 60 000 pounds show average losses of 1 600 pounds each; those testing from 60 000 to 64 000 pounds, 765 pounds each; those testing from 64 000 to 68 000 pounds, 4 520 pounds each; those testing above 68 000 pounds, 4 910 pounds each. In the series of tests in Table No. 1, bars testing between 64 000 and 68 000 pounds show average losses of 4 300 pounds each, but bars testing over 68 000 pounds lost 4 830 pounds each.

The same fact is even more clearly apparent with respect to elastic limits. In the identical series the average value of the elastic limit in specimen tests is just 38 000 pounds. Omitting No. 6 as doubtful, there are seventeen tests giving elastic limits below 38 000 pounds, and these seventeen bars include all but one of those which show gains in full-sized tests, the net result being an average gain of 86 pounds each. On the other hand, there are seventeen bars which give elastic limits above 38 000 pounds in specimen tests and these show an average loss in full-sized tests of 2 780 pounds each.

We have here, then, two related facts, viz.: 1st, in spite of the fact that practically every bar gave a lower ultimate strength in full-sized test, and some of them lost largely, yet each full-sized test gave figures which were acceptable to the engineer; and 2d, the large losses occur generally in conjunction with high figures in specimen tests, while the small losses occur in conjunction with lower figures.

These two points constitute a major and a minor premise on which to base a conclusion. Or rather they admit of two conclusions, one or the other of which must be verified by further consideration. Thus there is the very easy and obvious conclusion—(*A*) that we have only to use steel with low tensile strength and the large losses will disappear; and then there is the less obvious but more probable conclusion—(*B*) that the tests which gave high figures in specimen tests were less reliable than the ones which gave low ones, and did not in fact represent the mean value of the material.

If conclusion *A* were correct we ought to find (from the rate of progression indicated above), at about 56 000 to 58 000 pounds tensile strength, a grade of steel which would be practically a stable quantity. This is not only very unlikely from our general knowledge of how steel is affected by work and heat, but is refuted by such tests as we have of steel bars of lower tensile strength. Thus the following figures give the ultimates of all the good tests in two series of eye-bar tests made from steel which was required to show 57 000 to 64 000 pounds in specimen tests: 58 090, 61 150, 55 545, 54 480, 58 540, 54 510, 51 720, 55 850, 62 190, 57 260, 62 600, 59 750, 59 830, 53 790, 54 790, 55 460, 56 000, 54 000, 60 370, 49 970, 61 840, 55 350. These are clearly in about the same ratio to the requirements for specimen tests that the tests in Table No. 3 are to the specimen tests there. From a practical standpoint we know also that the softer metal is much more apt to have blow holes in it, and would consequently forge less satisfactorily.

Conclusion *B*, however, has much to commend it, because there are very good reasons why test pieces should generally give figures in excess of the mean or normal value of full-sized sections. A little cold rolling would produce this result in a test piece, and very often does so, especially in thin bars. Then it must be remembered that test pieces usually represent the best "cut" of the metal (if such a term may be borrowed from the shambles). It is a well-known fact that tests cut from certain parts of the cross section will give figures which are considerably higher, both in ultimate and in elongation, than tests cut from adjacent parts of the section. Moreover, the position of this "best" metal is definitely known, and there can be no doubt that our tests usually represent it, and consequently give results which are generally in excess of the average value of the full cross-section.

On the other hand, while it is reasonable to suppose that specimen tests may sometimes give too low values, such tests are likely to be few and their discrepancies small. Thus, if the test piece were not entirely sound the result would be too low; and so also it would probably be if the metal had been finished too hot in rolling. Clearly, however, the effect of these things could not be much in tests which would be considered satisfactory, a limitation which at once checks too low values. For these reasons the writer thinks conclusion *B* is the correct one, and argues that the reason why the bars which gave high figures in specimen tests show large losses, is because the specimen tests were too high; and that the tests which gave low figures in specimen tests are normal or occasionally a little too low.

This we may call the first element in the losses in eye-bar tests—too high values in specimen tests. The error is an undesirable one, since we are deceived by it, but it is clearly one which we can measurably control, by making a sufficient number of tests to get fair average values of the full sections, and by occasionally annealing specimens which are likely to have been cold rolled. Incidentally, however, it is to be noted as creditable to the steel makers, that a careful consideration of the facts shows quite a little of the irregularity to be due to accidental causes, and that the real quality of the material throughout the series is actually nearer the mean of the specifications than appears on the surface.

A second element in the loss in eye-bar tests, and the one most generally recognized, is due to annealing. All steel bars are, of course,

annealed after forging, and no one practically familiar with the matter would be likely to deny that this is important, or that it is desirable to do it thoroughly at a good uniform heat with slow cooling afterward. The results of the comparison in Table No. 4 of annealed and unannealed specimen tests are evidence that the annealing does lower the elastic limit and the ultimate strength of the steel. But the writer thinks these results indicate that the loss is not large. It will be noted that the maximum loss in the annealed specimen tests is about 2 800 pounds in elastic limit and 1 900 pounds in ultimate strength.

It is probable that annealing affects full-sized bars more than these figures indicate. The writer is credibly informed that some experimental tests of unannealed eye-bars gave high ultimates and thinks this likely to be so. The chief effect of annealing on the body of an eye-bar is to soften the hard and tough outer skin, and this would no doubt reduce its strength materially. Our comparison of results, however, is not between an annealed and an unannealed eye-bar, but is a comparison between a specimen test having two planed edges, and the annealed eye-bar; and on this basis both Table No. 4 and a number of tests in the identical series, offer evidence that the loss is not large when specimen tests represent the normal or average value of the material. But whatever the losses may be, due to annealing, it is to be noted that they are entirely legitimate and proper losses. The annealing is an important and valuable feature of the manufacture of eye-bars and any effect produced by it should be recognized and provided for in specifications. This point will be taken up again in discussing the practical deductions of the paper.

There remains a third element in these losses in eye-bar tests to be referred to, and it is the one which is the most objectionable—the bad feature in these losses. We may reckon 2 000 or 3 000 pounds decrease due to the too high results in specimen tests, and 2 000 or 3 000 pounds more due to annealing, and still not account for the losses which run up toward five figures. This third element, which, in the writer's judgment, is a feature of all large losses, is due to the two familiar facts: 1st, that in testing materials the fracture takes place at the weakest point; and 2d, that the range of values is likely to be greater in a long bar than in a specimen test. Hence, in testing a long bar we get a check on its uniformity—a most important matter, since homogeneous steel is the ideal metal, while a steel which is alternately hard

and soft in zones ought to be excluded from structural works. Now, in testing a bar which is not homogeneous, the "weakest point" is simply the place where the metal is the softest; hence, it is fair argument to hold that the figures obtained for elastic limit and ultimate strength, in testing such eye-bars, simply gauge these functions in the softest places in the bar. When large losses appear we know, therefore, that there was one place in the bar which was considerably softer than the test piece, and we are free to guess whether or not there may be places which are much harder. Hence, such results will bear investigation.

In this connection attention is called to tests 27 and 34, as they appear in Table No. 3, especially as regards their elastic limits. Tests 3, 20, 21, 24, 30 and 35 should also be noted carefully.

In this matter of hard and soft zones in the bars, the elongations in each foot of gauged length, which are frequently made a matter of record in eye-bar tests, would, no doubt, afford much interesting information if carefully worked up. Without attempting to do this at length now, there are given on page 373, by way of suggestion, several diagrams of bars in the identical series for which these measurements were recorded.

The most casual inspection will lead to the conclusion that bars 29 and 32 gave such excellent results because they were homogeneous.

This brings us to a consideration of the results obtained in Table No. 3, as regards elastic limit, which have previously been referred to in a very general way only. It must be confessed that they are less readily dealt with than the figures for ultimate strength, since the frequent gains in full-sized tests embarrassed us. It has been shown above, however, that the large losses occur in connection with high figures in specimen tests, even more notably in the case of elastic limits than in the case of ultimates. Table No. 4 gives us good reason to believe that annealing reduces the elastic limit in the same way that it does the ultimate. Lastly, as regards the uniformity of the metal, there can be no doubt that the elastic limit is an excellent criterion.

As regards the frequent gains shown in elastic limits, it should be observed that test No. 6 is probably in error, due either to reading the gauge or recording the figures incorrectly. This has been the view taken of this test from the time it was made, but it was, of course, impracticable to check the figures after the record was once made.

Now, the ordinary determination of the elastic limit, by the fall of the beam, in testing small specimens, is well known to be the least

accurate of our determinations. It is to be noted that the tests in Table No. 4 (in which annealed and unannealed specimens were to be compared, and for which, therefore, the determinations were rather more carefully looked to) show no gains in the full-sized bars. The determination of the elastic limit in eye-bar tests, on the other hand, is generally accurate, the point of yield being clearly and definitely marked on the gauge. The results for eye-bars are, therefore, more reliable than for specimen tests. Hence, it is likely that the frequent gains are due to inaccuracy in the specimen tests.

There is a suggestion in these occasional gains of 1 000 pounds to 2 000 pounds in eye-bar tests, that the eye-bar testing machine may have given too high results. This is quite likely true in some cases, and, when true, we must still further increase the losses in ultimates to get at the truth of the matter. The best eye-bar testing machines, however, when equipped with mercury gauges are believed to give quite accurate results. But regardless of these considerations, it is only fair to conclude from the identical series of tests, that it is an open question whether there is always a loss in elastic limits in full-sized bars. In other respects, however, the elastic limit results agree with all the conclusions reached above as to the causes of loss in ultimate strength.

To summarize, therefore, we find the losses in ultimate strength in full-sized eye-bars to be fully established as a fact, and to be due, if our view be correct, to a combination of three distinct causes, viz.:

1. Specimen tests which give results which are in excess of the average values of bars. This is the undesirable factor of these losses.

2. Annealing the eye-bars. This is the legitimate and proper element of loss.

3. Non-homogeneous steel. This is the bad element in the losses.

As regards the size of the bars, there is not much evidence that it affects the results. Thus, the average values in ultimate for eye-bars in the identical series are as follows:

				Per cent.
Area from	3 to 4	square inches,	average value......	96.2
"	4 to 5	"	"	95.5
"	5 to 6	"	"	98.7
"	6 to 7	"	"	94.8
"	7 to 8	"	"	93.1
"	8 to 9	"	"	95.6
"	10 to 11	"	"	93.1
	16 square inch area		....	85.4

In several of the items the number of the tests is too few to constitute a good average, but there appears to be a little tendency to larger losses in the bigger bars. If this is true, it is doubtless due to a greater lack of uniformity, since annealing would affect them rather less than smaller ones, and cold rolling would also be less likely to occur. There is, however, no demonstration on this head, and this leaves us with the three factors enumerated above as the elements which enter into the losses in eye-bar tests.

But one of these elements—the annealing—is at all likely to be determined within definite limits; the others will vary quite irregularly. This is the reason, as stated at the outset of the paper, why the tests cannot be plotted into a line or curve, nor can we predicate in advance how much a bar will lose in value when tested in full section.

Practical Considerations.—The fact that there is a perfectly legitimate loss in ultimate to be expected in a comparison of eye-bar tests with small specimens, is by no means generally recognized. The results obtained in this regard have frequently been an issue between manufacturers and engineers. Those, too, who have recognized that these differences were likely to exist, have been by no means clear in respect to the cause of them, or what latitude should be given to provide for them. If, as we now know, it is unreasonable to exact the same results that are obtained in specimen tests, must we then go to the opposite extreme, and hold the shops responsible only for the forging and annealing, and the mills responsible only for specimen tests selected by their own agents and prepared and pulled on their own machines? Something very like this is frequently urged as the proper course, but it is to be hoped that such a view will not prevail. We have already seen early in this paper that a majority of the specimen tests are inflated, and such tests under the conditions described above—which are the ordinary conditions—present few difficulties. The quality of our eye-bars is now maintained by the full-sized tests which are exacted, and we ought to provide for them under definite requirements.

What these requirements shall be must hinge on the grade of steel which is to be used. As regards this, the majority of the steel eye-bars which have been made in recent years have been required to show a mean ultimate strength in the specimen tests of 64 000 pounds, with an extreme range of from 60 000 to 68 000 pounds. The experience of the

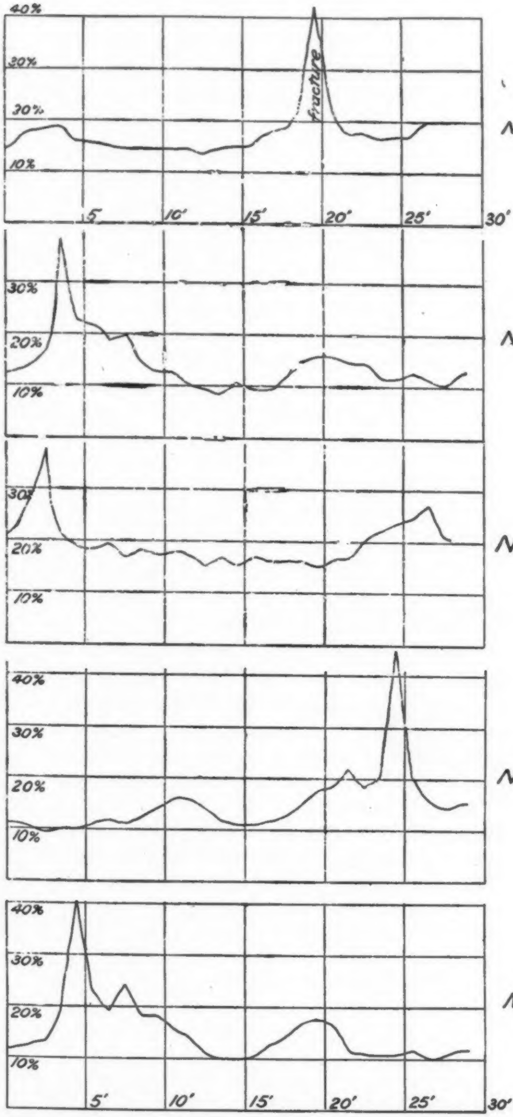
shops in forging this grade of material, as well as the large number of eye-bar tests which have been made, commends it as a thoroughly satisfactory quality of steel for the purpose, and probably better, all things considered, than any other grade of metal. It forges better than a harder steel and is less affected by small flaws, and it is sounder than softer metal. Adopting as a working basis, therefore, a steel of from 60 000 to 68 000 pounds, we can then proceed to deal with testing it in one of two ways. We can require the mills, in the first instance, to furnish enough specimen tests to demonstrate the fair average quality of the material and its homogeneity, and when this is done make full-sized tests to prove the forging only. Thus, if a specimen test shows but 60 000 pounds tensile strength, we are bound to accept it quite as readily as if it showed 64 000 or 68 000 pounds. But if we are to definitely accept the material, we are justified, in view of our analysis of the losses in eye-bars, in requiring manufacturers to demonstrate that it is a *bona fide* result, and that there is not 2 000 or 3 000 pounds in it due to cold rolling, 2 000 or 3 000 more due to selection of the test piece, nor soft spots in addition to reduce its value still further.

Or, in the second instance, we can deal with the matter by considering specimen tests to be matters of information only, and accept or reject the bars solely on the results of full-sized tests. This is probably the better way to deal with eye-bars, because the only really efficient test of homogeneity is the large bar, and because also the test of the full-sized eye-bar eliminates the elements which inflate specimen tests. Hence, by adopting the eye-bar test as the sole criterion, the manufacturer is required to use his ingenuity to get a genuine product, rather than to get a good specimen test. In deciding on a proper requirement for the ultimate strength of full-sized bars, it is necessary to reckon on 60 000 pounds as a basis in specimen tests, because, as pointed out above, we must accept a *bona fide* result of 60 000 pounds just as readily as 64 000 or 68 000 pounds. Our judgment of what reduction it is reasonable to allow must therefore be deducted from a basis of 60 000 pounds. What this "reasonable deduction" should be will depend to a degree on the judgment of individual engineers, since it admits of only approximate demonstration. From study of the question and from the examination of a large number of eye-bar tests extending over several years, the writer is disposed to favor 4 000 pounds

as a proper allowance to make, or 56 000 pounds, accordingly, as the minimum requirement for ultimate strength.

Whether we should include a definite requirement for elastic limit or not is perhaps open to question. The elastic limit is a function which is but little understood. Manufacturers must to a great extent accept what they get, having little control of their product in this regard. There are excellent reasons for believing that the normal elastic limit of 64 000 pounds steel is above 35 000 pounds, and is not much reduced by annealing. It is probably true, also, that those brands of steel bars which have commended themselves as the best in the market, are characterized by a high elastic limit. But it is very doubtful if the manufacturers of these bars could explain why this is so or how they achieve these high figures. Different processes of manufacture, too, seem to give different values in elastic limits for the same ultimates. Hence, it may not unreasonably be argued that the results obtained for elastic limit shall be free from limitation. On the other hand, the importance of the elastic limit as a criterion of homogeneity affords a strong reason for limiting it in full-sized tests. There are numerous tests on record which show losses in elastic limit of 10 000 to 12 000 pounds, the results in eye-bar tests running down to 30 000 pounds per square inch, or even in some cases as low as 28 000 pounds. In the writer's judgment such bars should be ruled out, and a minimum limit is necessary to do this, and is probable advisable. It should not, however, if adopted, exceed 32 000 pounds, since really good results are on record with elastic limits below 34 000 pounds.

If, as suggested, we are to accept or condemn eye-bars on full-sized tests, it will be necessary to make such tests for each melt of steel represented, as well as for the different sizes of bars. This means an increased number of tests (which are not, however, expensive), and if not looked after might lead to excessive testing. By requiring the manufacturers to limit the number of melts used in making the order, this could, however, readily be kept in check, and the number of bars need not exceed four or five per hundred as an average.



No. 29

No. 30

No. 32

No. 34

No. 35

DISCUSSION.

Percival Roberts, Jr., M. Am. Soc. C. E.—Mr. President and Gentlemen, I really hardly know what to say in opening a discussion of this character and of such magnitude. I have not carefully read the papers quoted from, but, as the Secretary read them, there were one or two points which occurred to me. One was, the effect of hammering upon steel. In this connection, a point comes up which possibly may be well known to every one, and yet, is one which I think deserves great attention; that is, never to hammer a piece of steel when that hammering will make the end concave instead of convex. We put a piece of steel under the hammer and do not give sufficient time for the effect of the blows to penetrate to the center; we consequently stretch the outside more than the center, producing very damaging results. This has not been taken into account in the manufacture of high-process steel, where the hammer has been used, and in nearly every case the effect of the blow has not been sufficient to penetrate the entire mass. We obtain much better results with the hammer-forging process, in which the effect of the blow is felt throughout the entire mass.

Another point which occurs to me is the number of specifications which the manufacturer is compelled to meet in the ordinary course of manufacture. A list was given me the other day of thirty different specifications for structural steel. In reference to mechanical results to be obtained, no two were alike, yet the same melt of steel would have filled all those specifications. If, however, you had asked the engineers who prepared them to accept any other specifications, they would have refused to do so.

The difference of which Mr. Lewis speaks, in the results between the specimen test piece and the test of the full-sized bar, is largely due to methods to which the manufacturer is compelled to resort in order to fill certain specifications where the elastic limit, in relation to the ultimate strength, is higher than it should be. The only way he can accomplish this, is simply to finish the piece at a lower temperature than he would do if the elastic limit was placed lower; the finished eye-bar is tested after annealing, with one result, and the specimen cut from cold rolled bars before annealing give different ones. But until some more uniform system is adopted these small differences must exist, and he must fill all specifications he meets. I am perfectly satisfied that if a more uniform method were adopted, manufacturers would be able to control their product better than they are now able to do with the constant variations they are compelled to meet. Each engineer believes that what he is doing is for the general good, and that his specifications are the best; but as I have said, the specifications that have to be met, while differing in small points, are on the whole, very uniform and can be filled with one average grade of steel.

In connection with oil tempering, I should say that oil tempering steel in steel castings has given very beneficial results, which approximate very closely to forging. If it could be done regularly, I would endeavor to oil temper all steel castings.

A. Gottlieb, M. Am. Soc. C. E.—When the Cincinnati Southern Ohio River Bridge was built, steel eye-bars were not known ; it is not so very far back, only in 1876, that such a bridge was built with even compression members of iron. The first steel bridge that I remember in this country with steel eye-bars, was the bridge over the Mississippi River, at Glasgow. General William Sooysmith was the Chief Engineer of that bridge. Mr. Linville was President of the Keystone Bridge Company, and knowing that steel eye-bars would be desired for the bridge then in contemplation, he made some experiments. He manufactured eye-bars, or pieces of steel that looked like eye-bars, by forging and upsetting them. I was in Chicago representing the Keystone Bridge Company, and I was informed that he had to give up the making of the steel eye-bars by either the forging or upsetting process. The next move was made by Mr. Andrew Kloman, who said that steel eye-bars could not be made in any other way except by rolling. He manufactured the first eye-bar actually used ; they were generally accepted, but the percentage of the good bars and bad bars was pretty nearly equal.

When I took hold of the Keystone Bridge Works in 1878, I came to the conclusion that I didn't see why steel eye-bars could not be made by the upsetting process. In steel there is no laminated fiber, it is of a molecular structure, and the particles are not distorted by upsetting as is the case in iron. I made experiments by upsetting steel bars, forming first a lump of the approximate shaping of a head, and then hammering it out in dies. I succeeded in making 5 and 6-inch bars. They gave uniformly satisfactory results. They were exhibited in Chicago in 1879, during the exposition of railroad appliances, which took place in that city. From that time on, eye-bars of the largest size were undertaken by upsetting the bars, and hammering them down and annealing them afterwards. What Mr. Roberts said about never hammering steel, I cannot agree with ; it depends altogether how you hammer ; if you have a proper weight of hammer and proper die, you may hammer steel all right.

As to annealing, I admit that eye-bars have to be annealed, but it is, in my opinion, a very crude process. You do something over which you have no control; you don't know how much good or how little you accomplish. You heat a particular eye-bar three or four times, another one twice only, one end has been to a yellow heat, the other one to a darker heat, the strains caused in cooling are entirely different from what they are in the body of the bar, so you anneal one part of the bar too much, or the other too little.

Mr. Roberts.—I think Mr. Gottlieb misunderstood what I said about the hammering process, never to use a hammer on a piece of steel which would produce a concave end on the piece that was being forged. If your hammer will produce a concave end it is too light; the moment you have a hammer sufficiently heavy to do the work you will find the ends in your steel will be convex, showing that the effect of your hammer has penetrated to the center of the mass. If your hammer is sufficiently heavy I do not consider that there is any objection.

Mr. Gottlieb.—I say it depends altogether on how the hammering is done. I took it referring to eye-bars.

Mr. Roberts.—I think our hammers have been too light; there are very few hammers heavy enough for that character of work; where you have a high vertical bed of material to work upon, the temperature is higher.

Mr. Gottlieb.—The eye-bars I made were made by upsetting; the hammering was simply to finish the head in the die to a perfect shape. The hammer was heavy enough to do the work. Ten-inch eye-bars with heads of 24 inches were made by upsetting and finishing under a 5-ton hammer.

James G. Dagron, M. Am. Soc. C. E.—The paper written by Mr. Lewis deals with facts quite well-known to those who have had anything to do with the testing of structural materials. I have had prepared, from the records in my office, a table showing the results of quite a number of tests of different size eye-bars manufactured from different grades of steel. This table gives the results of the specimen tests of pieces cut from the full-size bars and also the results of the tests of these latter, as well, in some instances, as the chemical analysis of the bars. With some few exceptions I do not find as considerable variations, as are shown in the tables given by Mr. Lewis in his paper, between the specimen test and the full-sized test.

The essential thing for an engineer in charge of important structures, is to know the quality of the material as it goes into the structure; therefore, full-size tests are of greater value to him. While it is also very interesting to know the condition of the material before it reaches its finished shape, this knowledge is of less practical value. If it were possible to anneal the small specimens cut from the full-sized bars, at the same temperature as these latter will be after being headed, it would be much preferable, in my judgment, to test them after such annealing; but if this annealing is done at the mills, is there any certainty that the full-sized bars, after the forming of the heads, will be annealed at the same temperature? This annealing of the small specimens must necessarily be done at the mills, if on the result of their test depends the acceptance of the full-sized bars and their shipment to the shops for final manufacture. In the method of testing small specimens

from the bars as carried out at present, there is a possibility that some of these specimens have undergone a partial annealing, if cut from bars which were surrounded by others in the process of cooling off on the hot bed; while others cut from bars which were on the outer edges of the hot bed during the cooling off have not been affected by the heat thrown out by adjacent bars to the same extent. This would account in part for some of the differences observed in the testing of the small specimens cut from bars made from the same heat of steel. If, as Mr. Lewis says, manufacturers could be brought to make eye-bars from as few heats or melts of steel as possible, by testing full-sized bars from each of these heats or melts, a better knowledge of the quality of the material could

Size.	Chemical Analysis.		Specimen.		Eye-Bar.		Difference.		No.
	C.	Ph.	E. L.	Ult. Str.	E. L.	Ult. Str.	E. L.	Ult. Str.	
4 x $\frac{3}{4}$	.21	.086	40 855	70 637	38 577	66 304	− 2 278	− 4 333	1
4 x 1$\frac{5}{16}$	.22	.091	40 315	71 293	40 799	71 398	+ 484	+ 105	2
5 x $\frac{15}{16}$	.23	.078	40 320	71 356	40 607	70 282	+ 287	− 1 074	3
5 x $\frac{15}{16}$	.23	.078	40 320	71 356	40 527	70 922	+ 207	− 434	4
5 x 1$\frac{1}{8}$	.18	.094	42 361	70 861	43 148	73 164	+ 787	+ 2 303	5
5 x 1$\frac{3}{16}$	.20	.081	41 476	71 587	40 582	69 825	− 894	− 1 762	6
6 x 1	.20	.086	41 595	72 886	39 541	69 197	− 2 054	− 3 689	7
6 x 1	.23	.090	40 109	79 872	48 937	75 240	+ 8 828	− 4 632	8
6 x 1$\frac{1}{8}$	.20	.084	40 727	70 729	43 535	70 947	+ 2 808	+ 218	9
6 x 1$\frac{1}{8}$	.22	.087	39 389	72 050	38 614	67 299	− 775	− 4 751	10
6 x 1$\frac{3}{16}$	.23	.090	39 861	71 233	37 923	70 210	− 1 938	− 1 023	11
6 x 1$\frac{3}{8}$	...	....	40 690	69 220	39 610	67 165	− 1 080	− 2 055	12
6 x 1$\frac{3}{8}$	.21	.090	39 602	70 455	41 244	69 488	+ 1 642	− 967	13
7 x 1	.20	.082	40 311	69 319	42 291	69 963	+ 1 980	+ 644	14
7 x 1	.18	.094	40 312	71 659	42 605	71 864	+ 1 293	+ 205	15
7 x 1	.17	.094	41 383	69 891	43 694	73 508	+ 2 311	+ 3 617	16
7 x 1$\frac{1}{8}$	.26	.080	41 110	70 011	48 948	74 587	+ 7 838	+ 4 576	17
7 x 1$\frac{1}{8}$	.23	.084	40 880	70 140	46 621	76 592	+ 5 741	+ 6 452	18
	C.	Mn.							
5 x 1	.23	.69	45 526	81 353	48 167	77 689	+ 2 646	− 3 664	19
5 x $\frac{13}{16}$	.23	.69	45 526	81 353	50 529	77 057	+ 5 003	− 4 296	20
5 x 1$\frac{3}{16}$	.23	.67	44 440	78 206	46 385	70 444	+ 1 945	− 7 762	21
5 x 1$\frac{9}{32}$	.23	.48	43 792	72 531	44 177	72 343	+ 385	− 188	22
6 x 1	.23	.57	41 774	74 000	44 204	74 037	+ 2 430	+ 37	23
6 x 1	.23	.57	41 774	74 000	42 462	73 383	+ 688	− 617	24
6 x 1$\frac{1}{16}$	.25	.49	44 254	74 816	40 994	69 931	− 3 260	− 4 885	25
6 x 1$\frac{1}{16}$	.25	.49	44 254	74 816	41 998	69 529	− 2 256	− 5 287	26
	C.								
4 x 1$\frac{1}{8}$	.07		43 120	66 248	41 900	67 400	− 1 220	+ 1 152	27
4 x 1$\frac{1}{8}$	.07		43 120	66 248	42 900	64 400	− 220	− 1 848	28
5 x 1	.07		38 067	64 102	37 377	64 597	− 690	+ 495	29
5 x 1$\frac{5}{16}$	.08		34 028	63 791	39 050	67 914	+ 5 022	+ 4 123	30
5 x 1$\frac{1}{4}$	.08		36 098	65 241	37 371	63 893	+ 1 273	− 1 348	31
5 x 2$\frac{1}{8}$	.11		37 444	64 407	37 926	57 067	+ 582	− 7 340	32
6 x $\frac{15}{16}$	.07		39 043	63 459	42 400	64 400	+ 3 357	+ 941	33
6 x 1$\frac{1}{4}$	.09		34 419	59 931	36 880	59 230	+ 2 461	− 701	34
6 x 1$\frac{7}{16}$	.10		31 420	62 021	39 129	64 645	+ 7 709	+ 2 624	35
6 x 1$\frac{7}{16}$	.10		31 420	62 021	39 239	66 405	+ 7 819	+ 4 384	36

be obtained; and, except in the case of very large structures, an excessive number of tests would not be required. This method, it seems to me, would be the proper one.

The fact that steel eye-bars give in full-size tests a lower ultimate strength per square inch than in the specimen tests, has been recognized in the specifications for bridge materials prepared by the writer for the Company with which he is connected, the steel for tension members being required, in the specimen test, to have an ultimate strength of from 60 000 to 66 000 pounds per square inch, while the full-sized test bars are required to have an ultimate strength of not less than 58 000 pounds per square inch, and these views have been confirmed by our experience. The writer has no recollection of any eye-bars having been rejected after full-sized tests on account of their ultimate strength being less than specified, which would tend to show that the allowance made is sufficient.

Size.	Specimen.		Eye-Bar.		Difference.		No.
	E. L.	Ultimate Strength.	E. L.	Ultimate Strength.	E. L.	Ultimate Strength.	
3 x $\frac{3}{4}$	37 370	66 280	43 110	67 340	+5 740	+1 060	37
4 x 1	37 370	66 280	42 970	66 260	+5 600	— 20	38
5 x $1\frac{1}{4}$	37 790	63 390	38 700	65 130	+ 910	+1 740	39
6 x $1\frac{9}{16}$	39 640	68 300	36 900	61 040	—2 740	—7 260	40
6 x $1\frac{1}{8}$	39 640	68 300	33 870	60 600	—5 770	—7 700	41
4 x $\frac{7}{8}$	38 480	64 370	45 850	64 100	+7 370	— 270	42
6 x $1\frac{1}{8}$	37 120	62 690	39 920	65 400	+2 800	+2 710	43
3 x $\frac{11}{16}$	37 790	63 710	39 650	59 150	+1 860	—4 560	44
6 x $1\frac{1}{16}$	39 500	60 310	36 270	62 020	—3 230	+1 710	45
7 x 1	37 350	60 640	36 800	61 130	— 550	+ 490	46
4 x $\frac{3}{4}$	37 280	67 120	44 200	67 620	+6 920	+ 500	47
6 x $\frac{15}{16}$	38 320	67 700	36 810	60 059	—1 510	—7 671	48
6 x $1\frac{1}{4}$	38 320	67 700	35 840	60 380	—2 480	—7 320	49

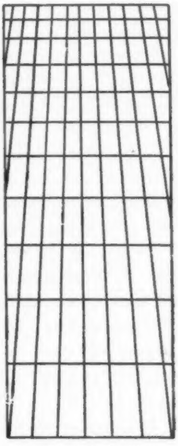
Note.—Nos. 1 to 18 inclusive, are Bessemer steel.
" 19 to 26 " " Open-hearth steel.
" 27 to 36 " " Bessemer steel.
" 37 to 44 " " Open-hearth steel.
" 45 and 46 " " Bessemer steel.
" 47 to 49 inclusive, " Open-hearth steel.

A. C. Cunningham, Assoc. M. Am. Soc. C. E.—In discussing the very valuable and interesting paper of Frederick H. Lewis, Esq., on the results obtained from tests of full-sized steel eye-bars, the writer will simply carry the subject further, and endeavor to show the causes of the losses or gains in ultimate strength in the full-sized test, as compared with the specimen test, and why the only rule that can be formulated is that, generally, there will be a loss of ultimate.

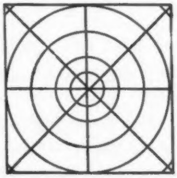
Some three years ago, the writer inspected a large lot of Bessemer

steel eye-bars, and the results of the tests then made and classified are given herewith. The $\frac{3}{4}$ Θ tests were from specially cast ingots, 4 inches square; the annealed specimen tests were cut from the opposite side of the same piece from which the unannealed test came, and the full-sized tests were taken at random from the finished bars. The tests in each group are, of course, all from the same blow.

This table shows that a loss of ultimate strength can be generally counted upon, but that how much it will be, cannot be accurately predicted from the size or thickness of the bar. Also, that an annealed specimen test will sometimes give a higher ultimate than the unannealed test, though nearly always with an increased stretch and reduction. The increase in ultimate in an annealed specimen is probably due to the manner of annealing; nearly always the specimen is heated in a forge fire, and then placed in lime; there is a sudden change of temperature when the piece is taken from the fire, and sometimes a slight chill when it is put in the lime, especially if the lime is somewhat damp. If the full-sized piece from which the test is to be cut, could be heated in a properly constructed furnace, and cooled slowly, without a sudden change of temperature, in a similar manner to the bars themselves, much more reliable and satisfactory results would be had.


SKETCH 1.

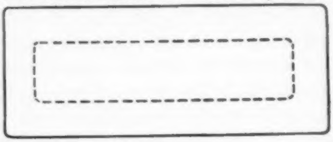
It is now a pretty well-established fact among steel makers that, as an ingot cools, the ingredients and impurities of the steel have a tendency to separate out, or segregate towards the center and top of the ingot.


SKETCH 2.

This condition of affairs may be illustrated by the sketches No. 1 and No. 2, which represent the vertical and horizontal sections of an ingot. The area inclosed between any four lines, will be inversely proportional to the amount of carbon, phosphite, etc., which will be found at that point, but as this segregation is a general law, and does not always take place exactly the same for each special ingot, the lines may become curved, and at varying distances from each other. The law of segregation is, however, a general one, and its effects upon specimen and full-size tests must become at once apparent. Suppose that a large ingot is rolled into a long bar, and that tests are cut from it, consecutively, from the bottom towards the top, the ultimate strength will be found to increase

from the bottom towards the top, and it would be expected to increase from the edge towards the centre. In plates, this latter will be found to be the case, but there are conditions of rolling in the eye-bar which may affect this result.

Suppose that a cross-section be cut from a large eye-bar, polished, and etched with acid (Sketch No. 3). It will be found that around the edges of this bar is a compact and well-worked shell of material, while the center consists of a more open and unworked material, so that, when we cut our tests toward the center of the bar, the increase of impurities may be more than counteracted by the unworked material.



SKETCH 3.

Specimen tests are taken from this well-worked shell of the eye-bar, and, consequently, give high results; and when it is further considered that the specimen test may come from near the top of the ingot, while the full-sized test may be from the bottom, it is little to be wondered at that there are, sometimes, large differences in ultimate.

Some of the practical conditions affecting specimen and full-sized tests of eye-bars, are the following: 1. Size of bloom from which rolled. 2. Rolled on universal mill or in grooves. 3. Number of passes in rolling. 4. Temperature at entering rolls, and at finish. 5. Temperature and time of annealing.

The subject of the manufacture and testing of eye-bars is an inexhaustible one, and it is to be hoped that manufacturers and engineers may unite in solving some of the more important problems concerning it.

Regarding elastic limits, the writer believes that as usually determined at the present time, they are not wholly reliable. The inertia and friction of the moving parts of a testing machine are variable factors, and the elastic limits indicated are too high. The elastic limit indicated for a full-sized test by the drop of the mercury column in the gauge of the testing machine, is about as accurate as anything we have.

The specification of a constant elastic limit for a variable ultimate, unless the minimum elastic is meant, should be avoided. A safe elastic limit to expect in the minimum case, that of a large section finished hot, is about 55 per cent. of the ultimate strength.

To avoid difficulties in full-sized tests, the specifications should either require the specimen tests from bars to pull a certain per cent. higher than the balance of the material, or provide for a certain per cent. of loss in the full-size test.

Comparative Tests.

$\frac{3}{4}$ Θ and Specimen Tests Annealed and Unannealed, and Full-Sized Bars Annealed. Specimen Tests cut from Bars as indicated by dark Portion of Sketch—

C. = Cup. S. = Silky. G. = Granular. E. = Edges. Cr. = Center. Ir. = Irregular.

Groups.	Kind of test.	Condition of test.	Test from.	Elastic limit per square inch.	Ultimate strength per square inch.	Per cent. of stretch. For specimens in 8 inches. For full-size bars in 12 inches.	Per cent. of reduction of area.	Character of fracture.
	Blow 26 016 ; Carbon .20 ; Phos. .063.							
1	$\frac{3}{4}$ Θ............	Normal	$\frac{3}{4}$ Θ	42 220	71 820	26.25	53.47	$\frac{1}{2}$ C. S.
1	Specimen	"	6 x $1\frac{1}{8}$	40 710	68 830	27.00	47.18	"
1	Full size	Annealed	"	36 500	62 100	43.70	32.60	G. E. S. Cr.
	Blow 25 979 ; Carbon .20 ; Phos. .065.							
2	$\frac{3}{4}$ Θ............	Normal........	$\frac{3}{4}$ Θ	41 900	66 440	28.25	58.96	$\frac{1}{2}$ C. S.
2	Specimen	"	6 x $1\frac{1}{16}$	41 570	71 400	26.25	50.08	"
2	Full size	Annealed......	"	40 700	65 200	41.90	49.60	S.
2	"	"	"	40 100	65 200	38.10	43.50	G. E. S. Cr.
	Blow 25 966 ; Carbon .19 ; Phos. .063.							
3	$\frac{3}{4}$ Θ............	Normal........	$\frac{3}{4}$ Θ	41 330	69 760	25.00	52.94	$\frac{1}{2}$ C. S.
3	Specimen......	"	6 x $1\frac{1}{16}$	39 780	69 460	25.75	44.31	45° S.
3	Full size	Annealed......	6 x $1\frac{1}{16}$	38 300	61 500	41.20	45.90	G. E. S. Cr.
3	"	"	"	38 300	65 000	42.50	46.00	" "
	Blow 26 031 ; Carbon .19 ; Phos. .069.							
4	$\frac{3}{4}$ Θ............	Normal........	$\frac{3}{4}$ Θ	42 440	73 640	25.00	55.86	$\frac{1}{2}$ C. S.
4	Specimen	"	4 x $1\frac{1}{16}$	40 880	69 400	25.00	48.41	45° S.
4	"	Annealed......	"	39 220	64 200	30 00	58.32	$\frac{1}{2}$ C. S.
4	Full size	"	"	40 600	67 100	36.00	45.00	G. E. S. Cr.
	Blow 26 025 ; Carbon .19 ; Phos. .062.							
5	$\frac{3}{4}$ Θ...	Normal........	$\frac{3}{4}$ Θ	41 880	74 470	26.25	53.37	C. S.
5	Specimen	"	5 x 1	41 480	72 320	24.50	46.78	$\frac{1}{2}$ C. S.
5	"	Annealed......	"	40 600	70 830	26.25	56.66	$\frac{3}{4}$ C. S.
5	Full size	"	4 x 1	42 100	65 000	36.60	48.40	G. E. S. Cr.

COMPARATIVE TESTS—(*Continued*).

Groups.	Kind of test.	Condition of test.	Test from.	Elastic limit per square inch.	Ultimate strength per square inch.	Per cent. of stretch. For specimens in 8 inches. For full-size bars in 12 inches.	Per cent. of reduction of area.	Character of fracture.
	Blow 26 021 ; Carbon .20 ; Phos. .058.							
6	$\frac{3}{4}$ Θ............	Normal........	$\frac{3}{4}$ Θ	43 570	72 720	24.50	54.48	$\frac{1}{2}$ C. S.
	Specimen	"	5 x $1\frac{3}{16}$	41 310	73 640	23.75	36.54	"
	"	Annealed......	"	41 850	74 440	23.25	49.67	$\frac{3}{4}$ C. S.
	Full size	"	5 x $1\frac{5}{16}$	43 700	57 600	45.60	50.00	45° S.*
	Blow 25 997 ; Carbon .19 ; Phos. .069.							
7	$\frac{3}{4}$ Θ............	Normal........	$\frac{3}{4}$ Θ	43 210	70 240	27.50	58.21	$\frac{1}{2}$ C. S.
	Specimen	"	6 x $1\frac{5}{16}$	40 370	72 060	25.00	40.00	45° S.
	"	Annealed......	"	38 780	72 260	25.75	43.46	"
	Full size	"	"	35 400	64 700	45.62	61.30	G. E. S. Cr.
	Blow 26 002 ; Carbon .19 ; Phos. .068.							
8	$\frac{3}{4}$ Θ............	Normal........	$\frac{3}{4}$ Θ	41 890	68 640	25.00	56.09	$\frac{1}{2}$ C. S.
	Specimen	"	5 x 1	41 900	76 700	25.75	43.76	"
	"	Annealed......	"	39 560	72 830	25 00	48.10	Ir. S.
	Full size	"	"	41 100	68 300	38.75	42.80	G. E. S. Cr.
	"	"	"	39 100	67 100	38.12	42.50	" "
	Blow 25 992 ; Carbon .18 ; Phos. .068.							
9	$\frac{3}{4}$ Θ............	Normal	$\frac{3}{4}$ Θ	42 020	69 390	28 75	57.14	$\frac{1}{2}$ C. S.
	Specimen	"	6 x $1\frac{5}{16}$	41 070	69 680	27.00	44.33	C. S.
	"	Annealed......	6 x $1\frac{11}{16}$	39 100	66 900	32.50	41.92	45° S.
	Full size	"	6 x $1\frac{5}{16}$	35 900	65 200	40.00	46.40	G. E. S. Cr.

WILLIAM METCALF, M. Am. Soc. C. E.—There are two reasons for offering the following report by Prof. Langley, instead of my remarks in reference to the two admirable and sensible steel papers which were under discussion at the last convention.

First.—It is a comprehensive, clear and valuable statement of all that we know about steel up to the present time; and although it could be written out to the length of hundreds of pages, such elaboration would not add to the report a single fact of value.

Second.—While my papers have been received by the members of the Society with flattering kindness, I have seen such criticisms as "dogmatic assertions should not be accepted as against scientific researches"; and again, "Mr. Metcalf's opinion is to be accepted in all matters relating to the behavior of steel, but scientific investiga-

* Borings from this bar gave .18 carbon.

tions as to causes are to be preferred to any opinions based upon shop observations, even the most intelligent."

Then the authors proceed to quote the work of one or another of the eminent scientists who have attacked the subject, one by one method, and another by another, and who have reached general conclusions from special investigations, which in the main are not completely proven, and even if they were clearly proved they would not cover all of the facts.

Personally, I have nothing to complain of, but I wish to convince the members, if possible, that my statements have been based, not on shop observation alone, but that they have been subjected to the most rigid and exact scrutiny by the most careful tests of both chemistry and physics.

As a chemist and physicist, I know no more able or reliable man than Professor John W. Langley; he has been engaged in the examination of steel for about twenty years, and has had at his command all of the appliances and apparatus of large university laboratories, and all of the facilities of a diversified steel plant. He has gone into every question *con amore*, without regard to the labor involved; he has hammered our shop opinions and prejudices unsparingly, and the conclusions now offered are as often the resultants of antagonistic views as of harmonious mutual work.

Surely, the man who was first to discover and announce that dissolved oxygen exists in iron and steel uncombined; who pointed out the variable expansion of steel due to variations in carbon and in temperature; who called attention to the fact that a true hardening effect may be produced by quenching from the heat of boiling water to cold water, a fact that has since been confirmed by beautiful and delicate magnetic tests at the Smithsonian Institute; who developed and used a beautiful and accurate quantitative method of determining nitrogen in steel; and who observed and developed the beautiful emery-wheel test of tungsten in steel, is to be ranked as a scientist whose opinion may be accepted when it is based upon so much and such long experience.

It is not intended by what has been said to contradict the statements of others; their conclusions may be true, and they may not. There may be a change from cement carbon to hardening carbon at the point of recalescence, but there is no proof of such a change.

There may be an α condition of iron below the heat of recalescence and a β condition above that heat, but that is not proved.

There may be a definite carbide of iron acting as a cement in hardened steel and an absence of the carbide in unhardened steel, but this carbide has never been separated and identified.

It is proved beyond controversy that there is a definite increase of

after quenching

hardness due to every increase of temperature from 212 degrees Fahr. to the point of granulation, the next condition below fusion.

The sudden increase of hardness at and about the heat of recalescence is explained by physical causes in Professor Langley's report so clearly and reasonably, that that explanation may be accepted safely until further research develops a better one.

It is proved also beyond question that there is a reduction of hardness caused by every increase of temperature up to the heat of granulation or even to liquidity, and that the amount of softness retained is a direct function of the slowness of cooling.

Accepting, then, the theory of solution as given by Langley, we assume that hardness is due to high tension, probably molecular, and that softness indicates the absence of tension; this makes a safe working hypothesis which all engineers will be secure in using until further and wider investigation develops something which is based on more convincing proofs than those offered.

One most important property of steel is not mentioned in the report. It is that a piece of steel registers in its grain always the last highest temperature to which it was subjected. This means that when a piece of steel is heated from a cold state to any color which can be seen by the eye, and then is cooled down, the resulting grain will be due to the highest temperature reached, modified, of course, by the mode of cooling and the treatment while cooling. But for similar conditions of heating, cooling, and working while cooling, the final result will be the same always.

This property is of inestimable value; it is so distinctive in steel cooled from fusion, that the carbon contents may be noted within .05 of 1 per cent.

This is only of value to the engineer as it is used intelligently by the manufacturer. But the grain of a forged or rolled piece of steel may be just as valuable a guide to any engineer who has to do with steel, and that means every engineer. If a fracture shows a uniform grain throughout, the mass has been subjected to a uniform heat and to uniform working.

If such grain be coarse and lustrous, of a yellowish caste, too high heat is indicated.

If it be fine, lusterless and steely blue or gray, a proper heat has been used.

If it be fine and black, or of a decided blackish caste, it has been worked too cold.

If the grain be decidedly uneven, the piece has been heated unevenly, or worked unevenly, or both. In this case injurious strains in the mass are inevitable, as it is shown that every variation of grain is accompanied by a variation in specific gravity, which means variation in volume, which means hurtful strains in the mass.

If the outside be fine grained and the center part be coarse and fiery, it shows high initial heat modified by superficial and insufficient working, either under the hammer or in the rolls.

If the inside be fine grained and the outside be coarse and fiery, it shows that the last heat was too high, too quick and superficial.

If the corners be coarse and fiery and the body of the piece be of proper grain, it shows carelessness in heating, allowing the corners of the piece to run up much hotter than the body.

All of the above indicate the value of annealing in every case where there is any reason to suspect unevenness of heating or working; for if any piece of steel be heated uniformly to, or a very little above, the temperature of recalescence, or as a good practical guide, to medium orange color, it will take the fine grain due to that color, and, if the heat be uniform throughout the mass, the grain will be uniform, and all strains will be reduced, if not eliminated entirely.

Finally, as to colors: the only cherry-red, if there be such a color, is that heat that just begins to show in heating up, or where the color begins to disappear in cooling. At this heat no work should be done on steel; it is a critical, dangerous temperature. Above this the true colors are, in ascending—orange-red, dark orange, medium orange, light orange, dark lemon, medium lemon, light lemon, or granular, or fusion. There is no true white color, unless liquid steel be overheated enormously.

PITTSBURGH, August 9th, 1892.

CRESCENT STEEL COMPANY,
Pittsburgh, Pa.

Gentlemen,—Pursuant to your desire to have for convenient reference a statement of some of the physical and chemical properties of steel, and of certain alloy steels, I have drawn up the annexed report. This is not intended to be a compendious collection of the existing state of knowledge on this subject, but rather to refer chiefly to experimental studies and practical observations made at the Crescent Steel Works, or in my own laboratory, and extending over a period of nearly twenty years.

During this time I have had the advantage of the co-operation of Mr. William Metcalf, whose knowledge and suggestions form a very important part of this report. We have worked so much together in this field that it is quite impossible in many cases to distinguish the personality of the one of us who is responsible for a particular opinion or a result; often they have been arrived by joint action; but wherever a result has been confirmed by mill or shop practice, this work has been carried on by him.

JOHN W. LANGLEY.

Report on Some Chemical and Physical Properties of Steel and of Alloy Steels.

For the purposes of this report, it is desirable to have a definition of steel from the chemical rather than the mechanical standpoint, because it takes up the subject mainly from the molecular and internal side rather than from its engineering and commercial one.

Steel, then, may be defined as iron holding in solution, in whole or in part, other elements within certain regulated limits. Cast iron and ferro-silicon are not included. The elements which it must contain to fulfill practical requirements are carbon and manganese. Those which it inevitably gets from the present processes of manufacture are silicon, sulphur, phosphorus, and, probably, oxygen, hydrogen and nitrogen. These are not essential elements, for their relative proportions and amounts may be varied, and indeed by special care one or two of them eliminated altogether, without depriving steel of its well-known qualities.

Besides the above there may be present, as accidental impurities, about fifteen other elements; generally, however, the quantities of these will be very small except in the case of alloy steels where they are intentionally added. The principal alloy steels are those of tungsten, chromium, manganese and silicon.

The alloy steels shade by insensible gradations into hard, brittle bodies, incapable of lamination. Throughout this report the word "steel" will be restricted to those metallic masses composed principally of iron, which are ductile and capable of successful working under the hammer or between rolls; also which possess initially or by sudden cooling a considerable degree of hardness greater than that of wrought iron.

The following table gives the approximate quantity of the more important elements occurring in steel:

TABLE No. 1.

Percentages of Elements.	Commercial Steels.		Alloy Steels.	
	Upper Limit.	Lower Limit.	Upper Limit.	Lower Limit.
Carbon	1.50	.30	2.25	1.25
Silicon	.30	.02	1.50	.50
Sulphur	.10	.005	.10	.005
Phosphorus	.10	.01	.30	.01
Manganese	1.00	.08	15.00	5.00
Tungsten	......	......	7.00	.50
Chromium	......	......	2.00	.25
Oxygen	.20?	Traces.	2.00	.25
Hydrogen and Nitrogen	Very little.	Very little.	2.00	.25

In Table No. 1 the lower limit indicates those quantities of the elements at which their specific action ceases to be sufficient to give the "alloy steel" special properties.

The lower limit of carbon in commercial steel is given at 0.30, because, below this, the metal becomes incapable of any notable amount of hardening when suddenly cooled.

The upper limit for oxygen is queried, because there are no wholly satisfactory methods of analysis for this element in the presence of large quantities of iron.

Hydrogen and nitrogen have been found in small quantities in all steels. Carbonic oxide appears likewise to be a universal ingredient of steel.

Since steel has been defined above as a solution of certain elements in iron, it may be proper to state here the meaning attached to the word "solution."

Chemical action appears to take place in at least two very different degrees of intensity. When it is exhibited in its maximum power and unopposed by other forces, it results in the production of chemical compounds, having, as every one knows, definite and fixed proportions of each ingredient. These are customarily spoken of as atomic combinations, it being assumed that they are formed by the juxtaposition of elementary atoms by a process of addition in which, necessarily, a whole atom is taken on each time. Experience shows that the number of such unions between any one pair of elements is quite small, generally one or two, and never exceeding seven.

To express this idea of definiteness of composition mathematically, consider the case of two elements, A and B, taken in the proportions by weight of x and y respectively. Then, in general, the compound, $Ax + By$, cannot be formed, for, as soon as x parts of A are taken, it will be found that the value of y depends only on Ax and B, so that $y = x\dfrac{A}{B}$ and the numerical value of $\dfrac{A}{B}$ depends on the elements chosen. In the case of hydrogen and oxygen, it has the value of one-sixteenth.

Moreover, if in the compound $A + B$ we make A constant, then we cannot vary the quantity of B by infinitesimal increments or decrements, for B also will remain constant until some simple multiple of itself is reached which will permit of the proportion $A + 2B$ or $2A + B$ being formed.

In chemical compounds of the above type, there is always a profound alteration of the properties of A and B considered individually.

Now, in the case of solution, the actions appear to be very different. When salt or sugar is added to water a kind of combination, certainly, takes place, for both of the solids disappear and will continue to do so till the point of saturation is reached; but this is not a fixed point, for

it varies enormously for most bodies with the temperature, and is also slightly changed by pressure. The phenomenon is characterized by a gradual modification of the properties of the solvent. Thus, the water grows progressively sweeter, or more saline. If gum is added to it, it becomes more and more viscid; hence, indefiniteness seems to be an attribute of the act of solution. To adopt the former notation, the compound (solution) $Ax + By$ can be formed for wide variations in the values of x and y. If one of the bodies is fixed in amount, then the other can be changed by infinitesimal increments and there will be a corresponding alteration in the solution as a whole. Finally, the change in the individual properties of the ingredients is gradual and usually not very profound. The nearer the reaction in dissolving a substance approaches the first or combinational type, the more complete will be the change of properties.

A complete theory of solution has yet to be formulated. Chemists and physicists are not agreed as to the kind and extent of the chemical actions which may take place, but all are agreed that the obvious visible phenomena of solution are different from those of typical chemical combination.

Now, looking at the behavior of steel, whether melted or solid, I am forced to regard it as exhibiting mainly the characteristics of solution, for fusibility and the property of hardening increase directly with the quantity of carbon added to it up to a certain limit, while its ductility decreases with the carbon. The analogies are very close in another respect; when crystallization from solution takes place, there is a strong tendency for the crystals to partially purify themselves by extruding foreign matter not necessary to their formation. Similarly, when steel solidifies, it always ceases to be perfectly homogeneous, the last portions to set containing an excess of some of the dissolved elements, notably carbon and phosphorus. This, which is called "segregation," is a very troublesome phenomenon and one which forbids us to hope ever to make, by the present appliances, the highest grades of tool steel in large masses.

Steel, then, is a solution of certain bodies in iron, and this definition applies to it, not only while it is melted, but also when solid, for changes in the distribution of the elements within a mass of steel can be produced while the metal is yet far from the liquid state.

In 1861 St. Claire Deville discovered the fact of dissociation, a process the opposite of chemical combination, whereby compounds may be resolved into their constituent elements.

Since then several independent lines of reasoning—thermal, electrical and chemical—point to the conclusion that all compounds when in solution tend to dissociate. I proved experimentally ("Proceedings American Association for the Advancement of Science," 1883), that even such stable bodies as the sulphates and chlorides of the alkalies and

alkaline earths showed evidence of decomposition when dissolved in a large volume of water. By analogy, therefore, it would seem that if definite carbides, oxides and phosphides were introduced into melted iron, they would tend to, and to a certain degree actually would, become dissociated and exist as dissolved bodies distributed throughout the iron, which would be modified by their presence.

Effect of Certain Elements.

When iron is exposed hot to the air it becomes superficially oxidized. If it is melted the oxygen penetrates the entire mass, although only in small quantity, and becomes dissolved.

This idea that oxygen may be dissolved in iron in contradistinction to existing as a definite compound, viz., oxide of iron, was stated publicly by me, and also for me by Mr. William Metcalf, in 1876. This view was not current at that time, but since then it has received the endorsement of a majority of metallurgists, largely because of the well-known reactions of overblown Bessemer metal and of "wild" open-hearth steel.

In some cases of imperfectly worked wrought iron, it has been possible to prove analytically the presence of as much as 0.30 per cent. of oxygen. In cast steel its detection is much more difficult. The effect of this dissolved oxygen is to make the steel very wild when fluid and red short when solid. It will crack under the rolls; in bad cases it will crumble and break, thus acting very much like sulphur, only apparently with more intensity. All those elements which have a strong attraction for oxygen tend to neutralize its influence; thus, silicon, aluminum and manganese, added to melted steel, diminish or abolish wildness and improve ductility. It is not probable that when oxygen is once dissolved in steel it can ever be completely eliminated, unless by returning the metal to the blast furnace or by converting it into cast iron. This last statement, however, must be taken as an opinion rather than a proved fact in the present state of metallurgical knowledge.

So strongly are small quantities of oxygen held by fluid iron, that, even when steel is melted in plumbago crucibles, and therefore is in contact with carbon walls and itself contains carbon and silicon, there is some degree of wildness even for the most perfectly melted metal, as can be proved by the addition of aluminum.

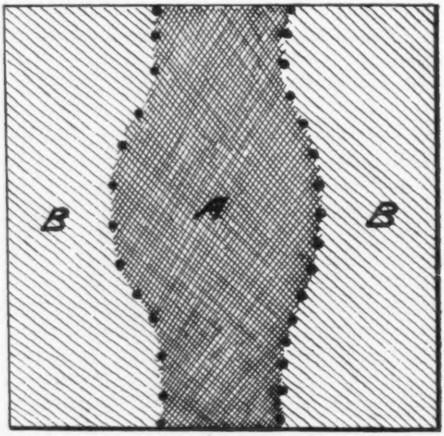
The influence of carbon is all-important, and in commercial steel it is the predominant one. To it alone steel owes its remarkable property of hardening on sudden cooling; its great tensile strength and elasticity are chiefly due to the presence of this element. The ideal steel would be composed only of iron and carbon. The nearer we can approach this condition, the better is the metal for all general purposes. All extraneous matter must be considered as a detriment; but actual con-

ditions of manufacture never furnish pure iron, and necessitate the intentional addition of at least one element—manganese—to partially neutralize the evil effects of sulphur and oxygen.

So much has been written on the influence of carbon, and there are so many theories of its action, that it would occupy too much space to attempt to review them here. It will be better, therefore, to confine this report to considering its function from the solution point of view.

Carbon will dissolve readily in melted iron; this can be ocularly shown, for if a piece of charcoal is forcibly held partly immersed in a bath of fluid iron, the charcoal will waste away from the under side, and the iron will show a gain in carbon by chemical analysis.

FIG. 1



The limit to the quantity it can thus take up is very near to 5 per cent. On cooling, a portion of this carbon will separate out, forming the graphitic carbon of cast iron and the semi-graphitic carbon of steel. This element is also dissolved when the metal is solid, and it can travel about in the interior of a bar by diffusion, in the same way that saline bodies can penetrate a gelatine film. The following experiment, performed at the Crescent Steel Works in 1889, shows this fact conclusively.

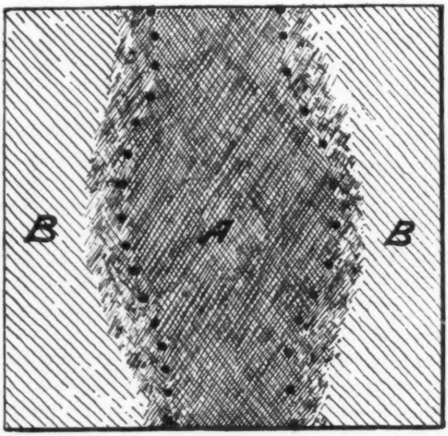
Mr. Metcalf and I took a piece of "hard and soft steel," that is, a compound piece made by inserting a bar of high carbon steel in the center of a mold and pouring mild steel around it, then rolling the compound piece into a 2-inch square bar.

After making a transverse section of it, the hard interior strip could

be plainly seen from the differential action of the cutting tool upon the two portions. On lightly etching the polished surface with dilute nitric acid, the more highly carbonaceous steel showed dark on a gray ground. The outline of the hard center was then pricked out with a center punch, sinking the holes rather more than a sixteenth of an inch deep. The section then had the appearance of Fig. 1, where *A* denotes the high carbon steel and *BB* the mild.

The piece was then closely wrapped in a sheet-iron covering and exposed for five hours to an orange heat, or somewhat below the melting point of copper. It was then cooled, the surface scale removed and the metal planed down nearly to the bottom of the punch marks. The carbon was found to have traveled outwards from the center to a distance of one-eighth to three-sixteenths of an inch where it formed a nebulous border, as shown in Fig. 2.

FIG. 2



That carbon could penetrate iron by the old and well-known process of cementation has long been recognized in steel practice, but it has been considered doubtful whether it may not have been carried as a gas, either as carbonic oxide or a hydrocarbon; but in the above experiment the carbon was in the center of a piece of steel and no gases could have been present unless they, too, were dissolved in the solid iron.

When carbon exists in iron to a greater amount than 2 per cent., and silicon is also present exceeding 1 per cent., then, if the metal is slowly cooled, a portion of the carbon separates out as graphite, leaving

a comparatively soft metal behind. This is the cause of the granular, or crystalline, brittle structure of pig and cast iron, where a soft metal sponge is filled with particles of graphite. This disposition of carbon to separate out in cooling is greatly promoted by silicon and is hindered by manganese. It is also favored by the length of time taken in cooling from the liquid state to that where crystals form, and the mass becomes semi-solid. Hence, the production of "chilled" rolls, shot, etc., is accomplished by pouring a metal rather low in silicon and containing a little manganese, with carbon over 2 per cent., against a cold surface. The period of cooling is thereby so greatly shortened that the carbon has not time to separate; it remains in solution and an intensely hard metal results.

If the disposition of iron to chill or of steel to harden is called K, and computed in terms of some standard iron, then the equation of hardening will be

$$K = \frac{C \times Mn}{Si} \times \frac{1}{t}$$

where the symbols C, Mn and Si do not stand for the atomic weights of carbon, manganese and silicon, but for these elements multiplied by coefficients not expressed in the equation; t is the time of cooling in seconds.

These coefficients are not yet fully worked out; they can be found only by experiment. Still they are known approximately; thus, that for carbon is from 2 to 4 per cent. of the weight of the iron, silicon from 1 per cent. to nothing for chilling irons. For steel the values are: carbon, from $1\frac{1}{2}$ per cent. to $\frac{1}{2}$ per cent. Silicon, under $\frac{3}{10}$ths of 1 per cent. Manganese is highly favorable to chilling and hardening; theoretically, its coefficient might be made very large, but other considerations come in which demand that this element in the case of steel should be kept quite low.

In steel the proportion of carbon is kept too low to permit of a "chill" being formed, in the technical use of the word, when the hot piece is rapidly cooled in water, but it does become exceedingly hard. The cause is the same in both cases, viz., a complete or nearly complete retention of the carbon in solution coupled with certain phenomena of crystallization and tension to be considered later. The difference between hardened steel and chilled iron is one of degree, not of kind. This view was, I believe, first put forth by William Metcalf over twenty-five years ago, and all the added information on the molecular behavior of iron gathered since that time confirms its correctness.

The converse of chilling is annealing; stresses are released by the action of heat, while carbon is thrown out of solution and tends to separate as graphite, only, in the case of steel, it never goes far enough to form true graphite. The equation of annealing, so far as it depends

on chemical composition, is the reverse of that given for hardening. It is—

$$N = t\,\frac{Si}{C \times Mn}$$

where N indicates the degree of softness or annealing in terms of a standard metal, and t the time in seconds during which the metal is cooling down from temperature T to temperature T'; these points being found by experience.

The best results require a careful regulation of the value of t. If it is too great and the metal kept too long at the upper temperature T, the steel will be over-annealed; it will somewhat resemble cast iron, and in extreme cases the carbon will have separated so far as to be visible to the eye in the form of black specks. Thus, we may compare the carbon partially thrown out of solution, and by over-annealing made to approach the graphitic state, to the particles of mud in muddy water. They are borne along mechanically, but do not affect the water except indirectly. Now, if mud had the property of dissolving when heated, so as to make the water transparent, but at the same time yellow, thick and viscid, the analogy with the behavior of carbon dissolved in iron would be very close.

The function of manganese is complicated. That it is practically necessary to have some of it in steel has been known for a long time. It is generally supposed to act by partially combining with the oxygen and sulphur, and thus withdrawing them from solution, so far as the iron is concerned. Undoubtedly it does this, but as the result of physical investigations confirmed by the practical observations of Mr. Metcalf, it is certain it has another important property also, which has been already referred to—that of promoting the solubility of carbon. So great is this power that, if the manganese is excessive, in the vicinity of 15 per cent., carbon cannot be removed by oxidation below ½ of 1 per cent. It favors hardening and opposes annealing; by retaining carbon in solution it promotes water cracking and makes the metal harsh and brittle; therefore, its use in steel must be confined to small fractions of 1 per cent. This function of manganese may be compared to a mordant in dyeing, where a color which would easily wash out may be fixed by first stamping on the cloth a mordant, which is a substance having simultaneously an attraction both for the coloring matter and for the cloth.

Besides these two properties, manganese is a hardening element of considerable power, so that even with very low carbon a steel too hard to be tooled results from mixing it with iron in quantities of from 10 to 20 per cent. Unfortunately, this steel cannot be annealed.

The action of sulphur is wholly detrimental; the same is true of phosphorus. Both of these elements have their injurious action increased somewhat in proportion to the amount of carbon present, so that for high steel they must be kept down to a few hundredths of 1

per cent. Sulphur has a notable influence in making iron and steel red short. Phosphorus greatly impairs ductility and tenacity in high carbon steel. In wrought iron its presence is much less injurious.

Silicon is generally objectionable, as it tends to cause an impairment of tenacity and ductility. Its action in this respect is not very marked if its amount does not exceed $\frac{4}{10}$ths of 1 per cent., while as a partial neutralizer of dissolved oxygen it may even be beneficial. R. A. Hadfield has published reports which state that silicon is an advantage up to a certain limit, but tests made by Mr. Metcalf and myself do not substantiate this view.

The influence of carbon, and to a less degree that of silicon, is shown by the following tests made by Mr. Metcalf on various steels:

Marks.	Carbon.	Pounds per Square Inch. Ultimate Strength.
1	Under .30	58 760
2	.50	87 726
3	.70	87 975
4	.70	108 095
5	Under .30	63 790
6	Under .30	73 887
7	.35	76 952
8	.62	84 500
9	.22	86 450

In Nos. 1 to 7 the silicon was about .20; phosphorus, under .05; sulphur, under .03. Nos. 8 and 9 contained less than .03 silicon, but they carried some tungsten and therefore belonged to the group of special steels. The last one especially shows that tungsten increases the tensile strength of mild steel, but the increased difficulty of working it largely neutralizes its benefits.

Alloy Steels.

There are forms of steel having special properties to fit them for particular purposes. Within our experience no addition of anything to iron beyond the limits of carbon and manganese, already given, improves it as a steel for all-round purposes. As soon as we specialize, it at once becomes evident that gain in one feature is met by losses in others. Hence, the number of special steels is small. The only useful alloy steels hitherto produced commercially, are four—tungsten, chromium, nickel and manganese steels.

It is popularly believed that tungsten renders iron very hard, and in support of this is the fact that there are many brands of so-called self-hardening steel on the market, that is, of a steel which does not require to be rapidly cooled in order to become file-hard. Nevertheless, this belief is erroneous, for if a steel be chosen not excessively high in manganese and carbon, as all the self-hardening specimens are,

then no amount of tungsten will make it file-hard if allowed to cool spontaneously in the air.

I have made alloys as high as 30 per cent. of tungsten which could be filed.

The true function of this element is to delay the rate of change of carbon when either going into or out of solution. It acts somewhat as glue in water would do towards the latter's power to dissolve salt or to permit it to crystallize out. The final solubility of the salt is not much affected by the presence of the glue, but the rate of dissolving is enormously lengthened.

In tungsten steel this results in giving to a lathe tool, for instance, the useful feature of taking a heavy roughing cut at a considerable peripheral speed which, of course, heats the tool considerably, but does not draw the temper, while a plain carbon steel tool would have some of its carbon thrown out of solution and become softened by so high a duty. The effect of the tungsten is, then, to require a change in the value of t. If, in the equation for hardening, t has its usual value of a few seconds, the tungsten steel will crack and fly to pieces; but if it is prolonged to a few minutes, as in spontaneous air cooling, then a hard product remains. If in annealing the usual time is taken, a tungsten steel will be imperfectly softened; but, if t is lengthened sufficiently, satisfactory results may be obtained.

Tungsten steel is neither so hard nor so strong as plain carbon steel; hence, there is no advantage in using it except for special purposes. An incidental result of the function of tungsten in delaying changes of carbon was discovered by me some years ago and is now known as the emery-wheel test.

If ordinary steel be touched to the surface of a revolving emery wheel, it will give off a shower of brilliant sparks which explode into smaller fragments after they have been projected from the face of the wheel. This appears to be owing to the combustion of the contained carbon; but if tungsten steel is put to the wheel it will give off only a dull red fire, free from brilliant exploding sparks. So powerful is this effect that less than 0.5 per cent. of tungsten can be readily detected by the use of an emery wheel.

Chromium, like manganese, is a true hardener of iron even in the absence of carbon; like manganese it tends to hold carbon in solution, but much more powerfully.

The addition of 1 or 2 per cent. of chromium to a carbon steel will make a metal which gets excessively hard. Hitherto its principal employment has been confined to the production of chilled shot and shell. Owing to the intense mordanting power of chromium the carbon is in very intimate and complete solution. Hence result powerful molecular stresses after cooling, and the shells frequently break spontaneously months after they are made.

In 1888, Mr. R. A. Hadfield, of Sheffield, produced a remarkable alloy steel, which he called manganese steel. It contained from 10 to 20 per cent. of this element, with carbon in some cases as low as 0.50. This material is initially nearly file-hard. Annealing does not soften it, but if plunged red hot into water it is slightly softened. Unfortunately this material, which has so many good qualities, is too hard to be tooled, and hence its applications are limited.

The same metallurgist has introduced a silicon steel, in which a large part of the carbon is replaced by silicon. This material is less subject to the injurious action of phosphorus than ordinary steel, but it does not surpass the latter in any useful physical property and falls below it in some others.

Some Physical Properties of Steel.

The most remarkable physical property of high steel, the one which gives it value as distinguished from iron and other structural metals, and the one which belongs to it almost exclusively, is that of becoming intensely hard when cooled rapidly from a temperature a little above redness. This subject has long exercised the minds of metallurgists, physicists and chemists. A check list of the literature of this subject would be very extensive. For the purposes of this report the subject will be confined to work done in connection with the Crescent Steel Company, and to the writings of William Metcalf and myself, followed by some recent articles published in England.

If a bar of high steel is broken by a transverse stress, the fracture will be rough and crystalline; this appearance is spoken of as the grain by practical steel-makers. It refers primarily to the broken surface and it does not assume that the internal arrangement in the undisturbed particles of the bar at a distance from the end will be identical with the appearance of the fractured surface, because, at the moment of rupture, the metal is subject to compressive and tensile strains which must have an important effect in placing the particles in the final state where they become visible on the surface. But while there is not identity of arrangement, still there will be a constant relation between the superficial and internal particles, so that it is legitimate to classify steel by its fracture. The term grain, then, relates to the crystalline structure of the metal, and it must be carefully remembered that it does not imply either a fibrous or a cellular structure. In fact, steel which has been thoroughly melted is wholly free from fiber or cells. Mr. Metcalf devised, in 1876, a simple but beautiful method of showing the dependence of the grain upon the temperature at which a piece of hot steel is cooled in water. His method is the following: Take a bar of steel about three-fourths of an inch in diameter and nick it with a chisel in six points about an inch and a half apart, and number them. Now, heat the bar so that the

No. 1 piece at the end shall be nearly white hot and scintillating, while No. 6 is not red hot, the temperature varying gradually between these extremes, and having approximately the following optical appearances: No. 1, scintillating; No. 2, yellowish white; No. 3, lemon yellow; No. 4, orange; No. 5, reddish orange; No. 6, black. Now, cool the bar in water and break it into six sections. Then the fractures will appear as follows: No. 1, coarse brilliant sandlike particles, very hard, but which crumble off readily. Probably the piece will be cracked down its side. No. 2, brilliant and sandy, but the grains smaller than No. 1; probably it will be cracked. No. 3, a brilliant gray crystalline background, showing sandy particles. No. 4, a very fine grained satinlike luster, the individual grains about $\frac{1}{5000}$th of an inch apart and wholly free from a sandy appearance. This is called the refining point. No. 5, like the preceding, but coarser and with a softened luster. No. 6, more decidedly crystalline, the grain coarser than No. 5, and the luster softened as though an infinitesimal film of oil was on the surface.

On trying the above with a file, No. 1 will be found glass-hard, but destitute of strength; No. 2, glass-hard and a trifle stronger; No. 3, very hard and moderately strong; No. 4 will scratch glass with difficulty, but is very strong and elastic; No. 5 can be filed, is very elastic; No. 6, soft. No. 4 gives the maximum of useful properties; it is that at which hardness and ductility are combined in the best proportions. This refining point is then a critical temperature condition, at which all steel should be hardened. It is not rigidly fixed, however, for it varies with the quantity of carbon in the steel. The above description applies to steel holding about 1 per cent. of carbon. The refining point will move up the temperature scale, *i. e.*, towards the hotter end, the lower the metal is in carbon.

The appearances noted above are intimately connected with the change of shape in the crystallization, *i. e.*, grain of the steel, and also with powerful internal stresses which are probably molecular in character; also with chemical differences in the amount of dissolved carbon. The evidence for this statement is ample. The changes in grain are directly visible to the eye, also they can be noted mechanically by the concomitant variations in hardness and ductility. As to the existence of internal stresses, the cracking of overheated pieces and the retraction of the edges of a ring of hardened steel after it has been broken show the fact clearly; also, there is a change of volume on hardening. This is an expansion, the amount of which varies with the quantity of carbon in the steel and the degree to which it was heated at the moment of plunging it into the water; it is sufficient in amount to decrease the specific gravity to a notable degree. In 1876, I made some tests of Crescent steel, the results of which were published in the "Proceedings of the American Association for the Advancement of Science," of that

year. The following table summarizes them. The first vertical column gives the numbers of a set of ingots differing from each other in carbon, but alike in other respects. The upper horizontal line gives the numbers of the nicked pieces broken off from rolled bars made from certain ones of these ingots, and heated at one end as has just been described. No. 5 was black hot, and No. 1 scintillating. In the columns below these numbers are the corresponding specific gravities:

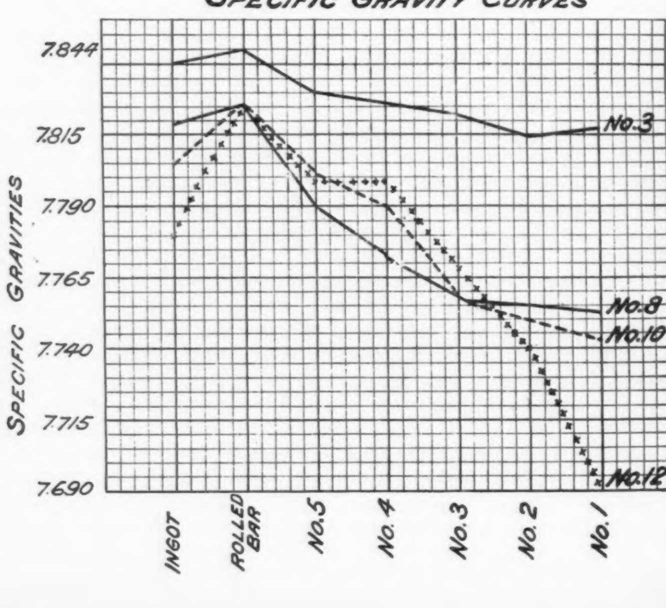
TABLE No. 2.

SPECIFIC GRAVITY TABLE.

	Specific Gravity. Ingots.	Carbon.	Bar. Rolled.	No. 5.	No. 4.	No. 3.	No. 2.	No. 1.
1	7.855	.302	......	....	....	....	....	....
2	7.836	.490	......	....	....	....	....	....
3	7.841	.529	7.844	7.831	7.826	7.823	7.814	7.818
4	7.829	.649	7.824	7.806	7.849	7.830	7.811	7.791
5	7.838	.801	......	....	....	....	....	....
6	7.824	.841	7.829	7.812	7.808	7.780	7.784	7.789
7	7.819	.867	......	....	....	....	..	....
8	7.818	.871	7.825	7.790	7.773	7.758	7.755	7.752
9	7.813	.955	......	...	....	....	....	....
10	7.807	1.005	7.826	7.812	7.789	7.755	7.749	7.744
11	7.803	1.058	......	....	....	....	....	....
12	7.805	1.079	7.825	7.811	7.798	7.769	7.741	7.690

FIG. 3

SPECIFIC GRAVITY CURVES



While the decrease in specific gravity varies with the increase in temperature in a general way, some of the horizontal lines show a few numbers differing from this, which is doubtless owing to the practical impossibility of heating all the pieces to exactly the same temperature.

In Fig. 3 the specific gravities for ingot and bars numbered 3, 8, 10, 12, have been plotted as curves. It is interesting to note that the effect of rolling has been to increase the density. Also that the decrease in specific gravity increases with the quantity of carbon, as shown by the way No. 12 pitches down to the base line while No. 3 is approximately horizontal. The refining point is that of the temperature corresponding to the figures in the column headed No. 4 at the bottom.

Mr. Metcalf has found the explanation of the liability of steel to crack in hardening in the expansion which it undergoes, as the above table of specific gravities proves.

The violent stresses set up by differences in the amounts of the expansion of hardening of parts of a bar due to overheating, or to unequal heating, account, in his judgment, for cracking, and lead to the practical injunction not to heat the steel any higher than the minimum necessary to harden it, *i. e.*, the refining point. In Fig. 3 the ordinate at 5 shows the state of the metal heated to an incipient red. This is enough to remove the increased density of the rolled bar and probably all internal strains. It shows the state of the steel at what may be called normal density. Now, the ordinate at 4 shows the refined and hardened metal, consequently the stresses due to hardening will be proportional to the differences between these two ordinates, which are slight; but if the steel has been heated to the temperatures corresponding to ordinates 3, 2 or 1, the departure from 5 is much greater, and hence a liability to rupture. It is very rarely that steel cracks at the refining point.

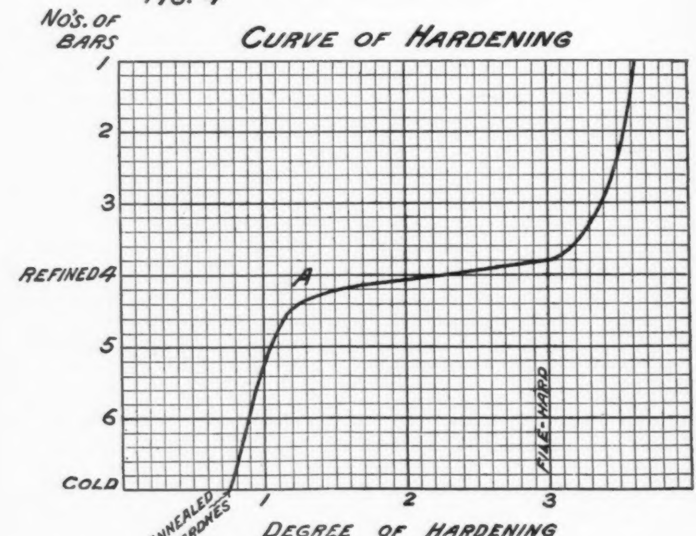
The degree of hardness also varies with the quantity of carbon and the temperature of cooling. The maximum rate of change from soft to hard occurs near or at the refining temperature, but it is not strictly confined to this point. If a bar is nicked and heated, as previously described, and then tested with a file, beginning at the cold end, a very slight increase in hardness can be felt till we come very near the refining point, when a very great increase occurs, and the file ceases to bite and slips over the surface. For all higher temperatures it will continue to slip, so that the bar seems equally hard up to the end which scintillated. But this is owing to the file being no harder than the test piece. If a diamond is used it will be apparent that there is an increase of hardness above the refining point. From tests recently made the following curve has been constructed.

In Fig. 4 the curve starts from the annealed bar. At *A* it begins to enter upon the refining stage. The curve now becomes nearly parallel to the axis of hardness. It is not possible to give quantitative

precision to the axis of hardening, because there are no trustworthy means for measuring this properly. The sensation to the hand when moving a diamond over the pieces of steel, together with a microscopic examination of the scratch, leave no doubt that the metal cooled from temperatures above the refining point sensibly gains in hardness, although the increase is not large. The chief gain in hardness is at, or near, the refining point. An angular fragment from piece No. 1 will scratch No. 4 more readily than it will scratch itself.

Closely connected with the subject of hardness are the changes in ultimate strength and elasticity due to hammering, annealing and tempering. The following table gives the result of tests made on some round steel bars, all from the same ingot, which were tested by tensile stresses, and also by bending till fracture took place.

FIG. 4



The total carbon given in the table was found by the color test, which, as is well known, is affected, not only by the total carbon, but by the condition of the carbon.

The analysis of the steel was:

Silicon	.242	Manganese	.24
Phosphorus	.02	Carbon	1.31
Sulphur	.009		

In this case the carbon was determined by combustion, and is, therefore, truly the total carbon.

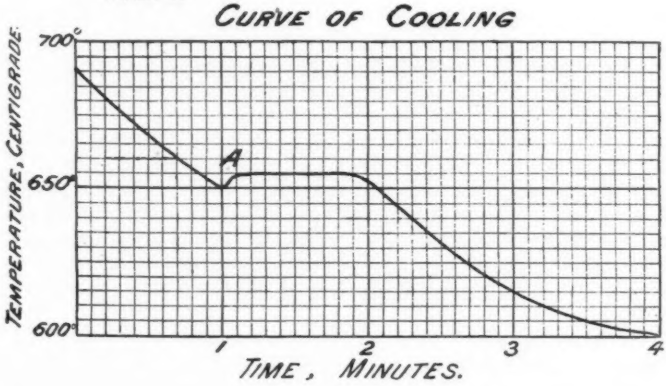
TABLE No. 3.

Nos.	TREATMENT.	Angle of cold bend.	CARBON.		Diam.	Elastic limit lbs. per sq. inch.	Tensile, lbs. per sq. inch.	Elongation, per cent.	Red area per cent.	Grain.
			Total.	Semi-graphite.						
		Degrees.								
1	Cold hammer bar..	153	1.25	.47	.575	92 420	141 600	2.00	2.42	Fine.
2	Bar drawn black.....	75	1.25	.47	.577	114 700	138 400	6.00	12.45	"
3	Bar annealed........	175	1.31	.70	.580	63 110	98 410	10.00	11.69	Fiery fine.
4	Bar hardened and drawn black.....	30	1.09	.36	.578	152 800	248 700	8.33	17.9	Fine.

RECALESCENCE.

It has been known for some time that if a steel wire is heated to a yellow-orange temperature and allowed to cool, its light will fade away till it is nearly black hot, *i.e.*, barely visible in a darkened room, when it will suddenly begin to glow afresh, and then fade away the second time. This phenomenon has been called recalescence. Recently, it has been examined by Osmond, of Paris, and Roberts Austen, of London, each observer using very delicate electrical pyrometers by which accurate registration of temperatures was accomplished. These observers show that recalescence is not confined to wires, but takes place in a mass of steel however large, only it is not readily exhibited to the eye, except in quite small wires.

FIG. 5



They show that if the pyrometer is inserted in a piece of steel which is cooling down from a high temperature, and the results laid off as in Fig 5, there will be an abrupt arrest of the descending pointer at one spot, marked *A* in the figure, as though the cooling had been stopped. This is the point of recalescence, which is at 655 degrees Centigrade according to Roberts Austen. What has really happened is this : the cooling goes on continuously, but at this point the sudden generation of heat from within balances the external losses, and so the pointer has only the horizontal component of its motion left.

Osmond has shown that a similar point exists for pure iron at 855 degrees C., only it is not so strongly marked. He thinks that this denotes a molecular change in the iron, while the 655 degrees point in steel indicates a change in the relation of carbon to iron. Roberts Austen has also pointed out that the temperature at which steel ceases to be magnetic is identical with the point of recalescence. He also says it is impossible to harden a piece of steel by plunging it into water at any temperature below the recalescence point.

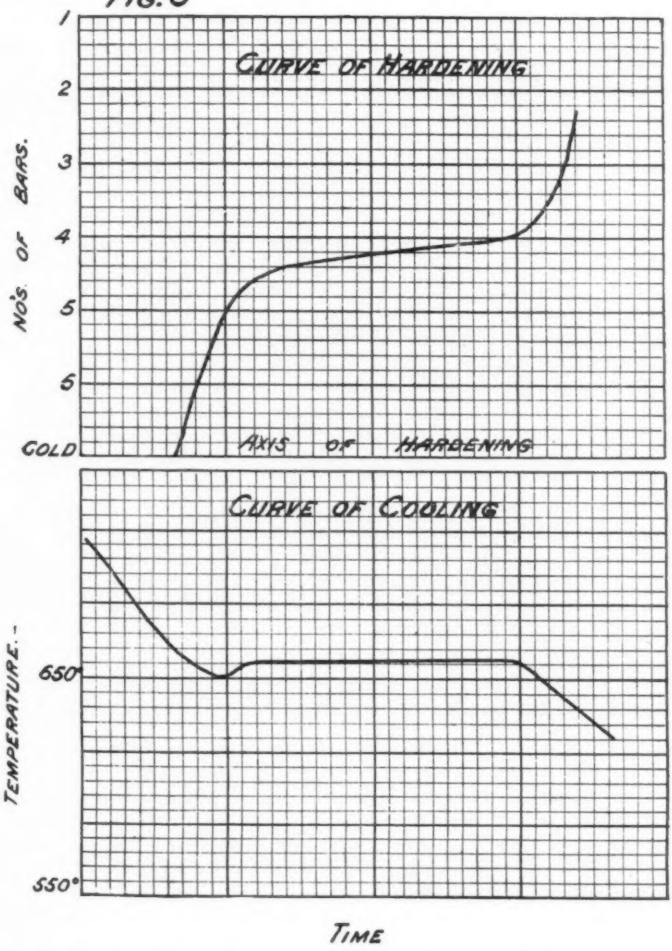
Very recently, in connection with Mr. Metcalf, some experiments were carried out which throw additional light on the phenomena of hardening. We heated by electricity wires varying in diameter from .035 to .250 of an inch, composed of steel holding 1.30 per cent. of carbon and very little of any other element. When using the smaller sizes the wires would cool down to nearly black before recalescence set in, the temperature then rising suddenly to an orange color and then fading slowly away. Moreover, if a cold wire was slowly heated up, there was a prolonged arrest at a dark orange color, after which a sudden apparent access of heat would set in and the wire would go rapidly on to higher temperatures. These phenomena make it possible to determine the point of recalescence very sharply by the eye alone, provided it has had some training in the estimation of temperatures.

We found that the refining point, which, as has been already stated, is the best temperature for hardening, was identical with the point of recalescence. This is a most interesting observation, for it shows that the refined grain, originally selected by the eye alone as guided by shop practice, is now proved to be that very remarkable stage in the heating of steel, where occur, in addition to the most useful degree of hardening, the loss of magnetic property and an important thermal change revealed by the pyrometer. The relation between the hardening and recalescence temperatures may be shown by combining Figs. 4 and 5, drawing them to the same temperature scale and placing one below the other.

In extending this work I encountered a new fact. If the wire is heated to a lemon color and then allowed to cool to nearly a black and

then plunged into water just before the recalescence rise takes place, it will be thoroughly hardened; but if it is heated from an initially cold stage to this same temperature, it will not harden. So this experiment proves that steel may be thoroughly hardened at much below the recalescence point.

FIG. 6



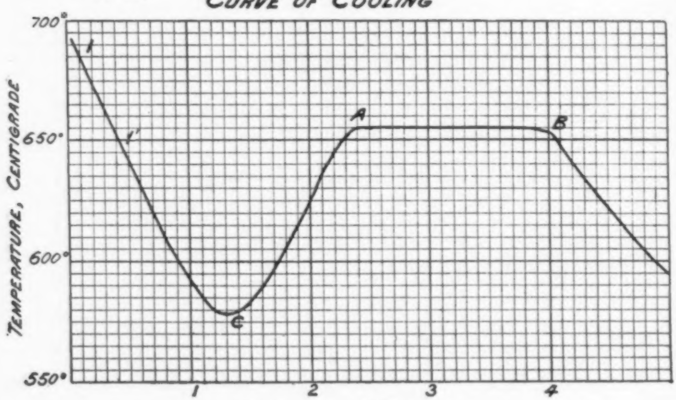
At first sight it seems as though this contravened what has previously been said; but it does not, if the following explanation is accepted.

During the heating of the wire just below 655 degrees, a breaking up of the crystals and a rearrangement of the particles takes place. Heat is rendered latent and is stored up, precisely as in the melting of ice. During the cooling of the wire from a high temperature, heat radiates away uniformly till the recalescence point is reached, when the stock of latent heat suddenly becomes available at or below 655 degrees, and a brightening of the color results. A small wire can part with its heat so rapidly that it can fall considerably below 655 degrees before the particles have had opportunity to move into their permanent or cold position. Hence, the potential heat is still in them, and hardening occurs to the same degree as though the sensible temperature was higher, because of the time lag. The capability of hardening is thus shown to be a function of molecular arrangement, not of heat. Hardening seems to be dependent on temperature, only because the latter is the best means of bringing about the favorable molecular condition.

The following curve, Fig. 6, shows the behavior of a small wire when cooling. The temperatures were estimated by the eye, taking Roberts Austen's recalescence point as 655 degrees.

FIG. 7

CURVE OF COOLING



I, temperature at which cooling begins. *C*, lowest heat attained by the wire and point at which recalescence begins. *A* and *B*, recalescence fully established and ended respectively. For this small wire, hardening can occur by sudden cooling anywhere from *I* to *B* on a cooling curve. A large piece of steel would not harden below 655 degrees, because that part of the curve *I' C A* could not exist for it.

Mr. Metcalf has previously called attention to four well-marked states in the thermal history of a cooling mass of steel. First, the liquid condition. Second, the granular state, when it is neither crys-

talline nor plastic nor ductile. Third, the plastic state, and fourth, the crystalline or solid. The change from the plastic to the solid state occurs at the recalescence temperature, and it is the reproduction of the crystals on cooling, with perhaps a change in the relation of the carbon to the iron, which causes the evolution of heat that is manifested as recalescence. A similar change, but less in amount, occurs in some of the alloy steels. It is said that nickel steel containing 25 per cent. of nickel is not magnetic at ordinary temperatures, and is relatively soft and ductile; but if it is cooled to — 4 degrees Fahr. it suddenly becomes magnetic with, simultaneously, a marked increase in tensile strength and a lessening of ductility.

Mr. Metcalf offers the following working hypothesis, with which I am in accord:

The solution of carbon in steel above the recalescence point is practically perfect unless the dose of carbon is very large. At the recalescence point and below it the excess of carbon tends to crystallize out; hence, very rapid cooling of the steel is necessary to retain it in solution. Also, the formation of large crystals is prevented, only small ones being produced, and forming the refined grain. This disturbance of the crystalline forces results in violent molecular stresses which produce hardness, for it is a familiar fact that cold rolling or hammering of any metal whatever, hardens it. The function of the carbon is to so alter the ordinary molecular aggregation of iron, that this strained grouping can be brought about within easily managed temperature limits.

Similarly, annealing is a partial throwing out of solution of the carbon previously dissolved.

The hardening or chilling of cast iron follows the same general law, only, owing to the greater amount of silicon and carbon present, the cooling must take place from the liquid condition.

To the above I would add this: the relation of carbon to melted iron is one which lies very near the border line separating typical chemical combinations from typical solutions, if, indeed, any such separation exists. Hence, the customary terms, framed to denote one or the other of these states, do not adequately express the shades of difference belonging to the actual phenomena.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INSTITUTED 1852.

TRANSACTIONS.

Note.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

554.

(Vol. XXVII.—October, 1892.)

EXPERIMENTS ON IRON AND STEEL JOINTS, RIVETED ON AN ANGLE.

By Bertram B. Flint, Esq.

Read June 10th, 1892.

The point of the experiments here described arose from observation of the fact that the line of fracture in a riveted joint with a double row of staggered rivets almost always follows the diagonals. That is in the sketch, Fig. 1, instead of the break extending directly from *a*

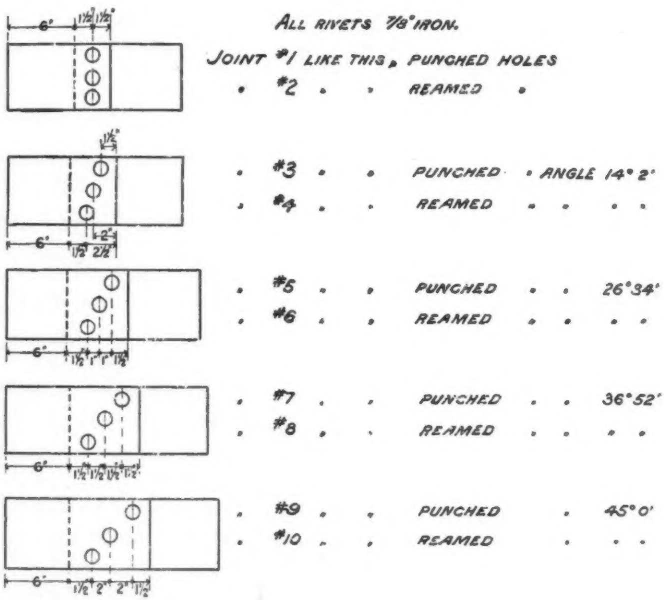
Fig. 1

Fig. 2

to *c* and *c* to *e*, it will go from *a* to *b*, thence to *c*, *d* and *e*. Now, what will be the failure of a lattice bar in a lattice girder? Will the bar break straight across from the rivet *f*, Fig. 2, or will the fracture take place at an angle running from rivet to rivet? In other words, the

problem is : what angle must a line of holes make with the edge of the bar so that the member shall be as ready to fail by a fracture at right angles to the edge of the bar as by a fracture running from hole to hole ?

In order to find out something about this a preliminary set of experiments was made as follows : 6 x $\frac{1}{2}$ inch iron plate was made up into ten riveted joints, which appear below.



In the case of the joints with the punched holes the size of the punch was $\frac{15}{16}$ inch ; die, 1 inch. In the joints with reamed holes the punch was $\frac{11}{16}$ inch; die, $\frac{3}{4}$; reamer, $\frac{15}{16}$ inch. The results appear as follows in a table : *

* In testing the specimens, the ends of the 6-inch plates were gripped flatwise, and a tensile strain applied in every case. The "angle" referred to in the paper was the angle made by the line of rivets with a line at right angles to the edge of the 6-inch plate.

No. of experiment.	Breaking load of joint.	Breaking load per square inch, net section.	Per cent. stretch in 2 inches.	Fracture.
1	72 900	45 800	10.5	Plate broke between holes.
2	82 500	50 000	22.5	Rivets sheared.
3	75 000	47 410	14.0	Plate broke between holes.
4	83 300	50 700	9.5	Rivets sheared.
5	78 500	48 850	10.5	Plate broke between holes.
6	83 600	50 100	14.0	Rivets sheared.
7	79 100	55 660	20.0	Plate broke between holes.
8	80 000	54 500	24.0	Rivets sheared.
9	86 600	50 640	16.0	" "
10	83 000	50 120	18.0	" "

In Nos. 7 and 8, the iron used by mistake measured $\frac{45}{100}$ inches thick, instead of half an inch, as in the others, which accounts for their higher ultimate per square inch of net section. Several points in the above results are worth notice.

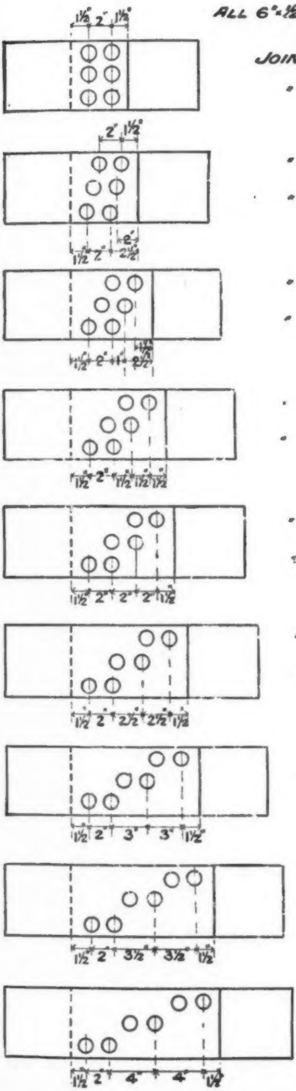
First.—The marked superiority of the reamed specimens.

Second.—The increase of strength per square inch of net section as the angle increases.

Third.—The strength of the joints having the same kind of holes, is about the same whether the holes are in line straight across the plate, or at an angle. So that in figuring the strength of the connection, it appears that three times the diameter of the rivet should be subtracted in all cases, as well as in Nos. 1 and 2. The net section was taken as the width of the particular plate less three times the mean diameter of the holes, multiplied by the thickness. This enables a comparison to be kept up of the strength of equal sections of metal. The results of Nos. 9 and 10 determined the writer to extend the series, in order to see what would happen if the rivet section were made great enough, by putting in more rivets, to break the plate.

In the following the holes were all reamed $\frac{15}{16}$ from $\frac{11}{16}$ punch. The forms of the joints appear on next page :

ALL 6" × ½" PLATE, ALL RIVETS ⅞" DIA.



JOINT # 11	LIKE THIS;		IRON		
"	12	" "	STEEL		
"	13	" "	IRON, ANGLE		14° 2'
"	14	" "	STEEL	"	" "
"	15	" "	IRON	"	26° 34'
"	16	" "	STEEL	"	" "
"	17	" "	IRON	"	36° 52
"	18	" "	STEEL	"	" "
"	19	" "	IRON	"	45° 0'
"	20	" "	STEEL	"	" "
"	21	" "	IRON	"	51° 21'
"	22	" "	STEEL	"	" "
"	23	" "	IRON	"	56° 19'
"	24	" "	IRON	"	60° 15'
"	25	" "	IRON	"	63° 26'

The results of these joints are tabulated below:

No. of Expt.	Breaking load of joint.	Breaking load per square inch, net section.	Manner of fracture.
11	95 000	60 430	Plate broke between holes.
12	156 000	101 300	" " " "
13	94 200	60 460	" " diagonally between holes.
14	149 800	98 500	" " " " "
15	99 000	63 750	" " " " "
16	160 000	101 800	" " " " "
17	97 300	64 180	" " " " "
18	158 400	102 800	" " " " "
19	93 700	61 430	" " straight across.
20	158 000	102 600	" " between holes.
21	102 100	66 300	" " straight across.
22	164 000	108 500	" " " "
23	93 800	59 670	" " " "
24	98 400	62 590	" " " "
25	97 000	61 700	" " " "

Nos. 11, 13, 15, 17, 19, 21, 23, 24, 25, iron.

Nos. 12, 14, 16, 18, 20, 22, steel.

Specimen tensile tests of the material used in these experiments showed—

	Elastic limit.	Ultimate strength.	Stretch.	Reduction area.
For iron....	30 900	49 880	18.7	24
For steel ...	41 680	67 650	27.3	54

By a comparison with the results of Nos. 1–10 it will be seen that the ultimate per square inch of the iron in these last experiments is higher than in the others. The reason for this is that these latter have reamed holes. Where the others have reamed holes the failure was by shearing the rivets so that the full strength of the iron plate was not developed—

The same thing is noticeable in this table as in the other, namely, the rise of ultimate per square inch of net section as the angle increases. This is what we should expect—to find the value rising from 60 430 in No. 11 to a maximum of 66 300 in No. 21—when fracture took place straight across the plate. The steel likewise increases from 101 300 to 108 500 when breaking straight across. From the above results we

would be warranted in thinking that a lattice bar at the usual angle would be as ready to break across as through the holes. But the method of calculating the allowable load in such a bar commonly used is erroneous. Taking the tensile strength of the iron at 49 880, and deducting one rivet hole according to custom, we should get for the strength of the connection:

$$(6 - 1) \times \tfrac{1}{2} \times 49\,880 = 124\,700.$$

But the highest value for iron (No. 21) is only 102 100. Thus, we see we are loading the metal 22 per cent. higher than we intend, and if we figure on 10 000 as the greatest allowable stress per square inch we are really straining to 12 200.

With the steel the case, curiously, is very different. Computing as before, we should get $(6 - 1) \times \tfrac{1}{2} \times 67\,650 = 169\,800$ as the strength of the joint. We actually get 164 000 in No. 22, so that the overloading is only about 3 per cent. For the iron it would seem that subtracting two rivet holes would give safe practice.

$$(6 - 2) \times \tfrac{1}{2} \times 49\,880 = 100\,000.$$

No. 21 gave 102 000, so we are on the safe side for this angle. If the angle is less than 45 degrees, more than two holes obviously should be deducted.

Lastly, it will be seen that while the iron breaks straight across when 45 degrees is reached, the steel does not until 51 degrees. The reason for this may be about as follows: As the ratio of the shearing strength of iron to the tensile strength is 80 per cent. while in steel of this grade it is only about 75 per cent., the iron will reach a point where the metal between the holes (which is chiefly subjected to a shearing action) will be sufficient to force the plate to break across normally sooner than the steel, which requires more metal in proportion, to bring its shearing resistance equal to its tensile resistance. Hence, more metal implies a larger angle, which is what is found.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

555.

(Vol. XXVII.—October, 1892.)

HOT TESTS FOR DETERMINING CHANGE OF VOLUME IN PORTLAND CEMENT.

By W. W. MACLAY, M. Am. Soc. C. E.

READ MAY 18TH, 1892.

WITH DISCUSSION.

The most important quality in Portland cement is its constancy of volume, and this should take precedence over the early tensile strength; because, no matter how strong a cement may be at first, if it shows any tendency to swell or change its volume, it is only a question of time when it will eventually fail by the disintegration of its constituents. The object of this paper is to show that the test generally employed heretofore to determine the change of volume in Portland cement is of very little practical value and should be changed to a steam and hot-water test, which does show quickly the presence of disruptive forces in the cement, if they exist at all, and which will eventually cause its

destruction, sometimes at an early and sometimes at a later period. The necessity of this test and of its being correctly made will be more readily recognized, since it can be shown that a cement that is liable to change its volume and ultimately to disintegrate, may in its tensile strength for short periods be all that is desirable, or even be excessively strong. It can also be shown that there is no other test or requirement for Portland cement, nor any chemical analysis, that will show its liability to failure in the work so clearly and sharply as the steam and hot-water tests.

This method of testing for change of volume first occurred to the writer, in testing some cement containing a large quantity of free or uncombined lime which was not shown at all by the ordinary cold-water change of volume test. The well-known avidity of pure or caustic lime for water suggested placing the pats in a steam bath in order to hasten the slaking process, and this was attended with such quick and satisfactory results, that since that time it has been the writer's practice to treat all cements in this way to determine their liability to change their volume.

This test as at present carried on in the testing of cement by the Department of Docks of New York City,* consists in moulding six pats of pure cement and water, about one-half inch thick and about 3 inches in diameter, on thin glass plates, and of the same consistency as for the briquettes for tensile strength. One of these pats is placed in a steam bath, temperature 195 to 200 Fahr., as soon as it is made. The second pat is placed in the same steam bath as soon as it is set hard, and can bear the 1-pound wire. The third pat is placed in the steam bath after double the interval has elapsed that it took the pats to set hard, counting from the time of gauging. The fourth pat is placed in the steam bath at the end of twenty-four hours. The fifth pat, as soon as it is set hard, is placed in fresh water of a temperature of about 60 degrees. The sixth pat is kept in moist air at a temperature of about 60 degrees. The first four pats are each kept in the steam bath three hours, then immersed in water of a temperature of about 200 degrees Fahr. for twenty-one hours each, when they are taken out and examined. To pass this test perfectly, all four pats, after being twenty-one hours in hot water, should upon examination show no swelling, cracks nor distortions, and should adhere to the glass plates. The latter requirement, while it

* See Plate LI.

TABLE No. 1.

No. of Test.	Setting time to bear 1 pound wire $\frac{1}{24}$-inch diameter.	Fineness per cent. passing sieve 2 500 mesh.	Neat Cement. Average breaking weights in pounds per square inch in tension. Immersed in hot water after first 24 hours: 2 days old.	3 days old.	4 days old	7 days old.	Normal cold test: 7 days old.	28 days old.	1 Cement, 2 Sand. Average breaking weights in pounds per square inch in tension. Immersed in hot water after first 24 hours: 2 days old.	3 days old.	4 days old.	7 days old.	Normal cold test: 7 days old.	28 days old.	Behavior of cement pats steamed and boiled.
1	120 minutes.	97	282	344	440	602	406	....	102	142	186	260	155	....	Good.
2	60 "	99	86	118	152	207	334	....	77	68	92	128	156	....	Bad.
3	180 "	97	402	476	468	544	466	....	148	170	156	188	186	....	Good.
4	240 "	98	260	328	352	426	372	....	105	130	156	180	176	....	Good.
5	10 "	94	214	294	386	542	366	....	94	142	174	178	158	....	Bad.
6	240 "	98	94	202	288	406	297	426	71	118	134	168	158	224	Bad.
7	10 "	92	100	155	208	344	272	350	59	93	116	151	129	178	Good.
8	15 "	94	64	106	150	212	360	498	81	102	137	147	140	237	Bad.
9	10 "	93	128	241	268	432	290	428	69	114	129	146	134	200	Good.
10	10 "	95	92	153	199	264	319	414	44	59	80	91	134	178	Fair.
11	10 "	95	130	184	230	326	326	454	53	84	83	111	130	204	Good.
12	15 "	84	181	268	382	456	290	402	43	58	82	100	93	130	Good.
13	180 "	99	452	460	510	604	520	644	128	140	150	162	274	378	Good.
14	10 "	99	346	432	482	510	492	594	102	128	128	144	214	314	Good.
15	10 "	95	76	99	125	196	341	532	62	75	83	118	146	176	Bad.
16	10 "	95	62	109	184	260	358	463	58	73	102	125	135	177	Bad.
17	10 "	97	49	70	103	163	362	440	53	83	88	132	136	199	Bad.
18	10 "	96	9	14	27	68	308	400	33	44	53	71	95	151	Bad.
19	240 "	99	72	144	200	318	373	449	54	80	104	148	147	192	Good.
20	240 "	99	428	571	628	673	557	678	173	198	222	201	229	340	Good.
21	10 "	96	6	17	31	54	310	350	35	55	74	89	133	190	Bad.
22	10 "	96	213	340	391	478	425	518	61	85	105	133	137	204	Good.
23	10 "	97	68	124	218	332	386	507	70	92	131	150	147	248	Good.
24	4 per cent. quick lime added to cement No. 20		49	122	144	209	515	612	....	....	....	....	....	....	Bad.
25	2¾ per cent. quicklime added to cement No. 20		....	....	....	....	....	....	123	155	174	186	272	332	Bad.
26	2 per cent. quick lime with cement No. 20..		279	372	435	512	592	656	121	127	146	179	259	342	Bad.
27	5 per cent. quick lime with cement No. 20..		20	39	58	72	451	620	22	37	48	65	233	354	Bad.

In the hot-water tests, the briquettes were allowed to set 21 hours in normal moist air, 60 degrees Fahrenheit; then 3 hours in steam bath, about 195 degrees Fahrenheit; then immersed in water of about 200 degrees Fahrenheit until breaking. The averages are all from breaking sets of five briquettes. The samples, from which each test was made, were generally taken from 500-barrel lots.

obtains with some cements nearly free from uncombined lime, is not insisted upon; the cracking, swelling and distortion of the pats being much the more important features of this test.

In hot water tests, where the cement is very objectionable from excess of free lime, improper burning, or other causes, the trouble generally shows itself in the cracking or distortion of all four pats. Where the cement is not so bad, the cracking and swelling takes place in the first three pats only, and when the cement is still less objectionable, only the first two pats crack or swell. The cracking or swelling of No. 1 pat alone can generally be disregarded.

In every case of failure and rejection the cement should have been allowed to set hard in a normal temperature before subjecting it to a steam bath. While the trouble developed by the hot water test is, nine times out of ten, an excess of free or uncombined lime, it is not to be inferred that this is alone the cause of the cement pats failing to preserve their volume. A cement properly proportioned as regards the raw materials, but where the burning has not been carried to nearly vitrification, while it contains hardly any free lime will fail in the hot water test by swelling and distortion. This brings up the question of the infallibility of the hot water test, and while the writer does not contend that it is absolutely so, he does think that the failure of a cement to pass this test throws a grave suspicion upon its quality, and fully justifies its rejection, especially when it is corroborated by the low tensile strength of the briquettes, gauged with neat cement and two parts of sand, steamed first and then kept in hot water of the same temperature as the pats.

These briquettes are prepared and treated, as follows: when making the briquettes for the ordinary cold water tests, four additional sets of five each of neat cement, and four additional sets of five each of mortar, one part cement and two parts sand, are prepared, and allowed to set twenty-one hours in normal moist air of about 60 degrees Fahr. They are then placed for three hours in a steam bath, about 195 degrees Fahr., then immersed in water maintained at 200 degrees Fahr., after which they are broken when two, three, four and seven days old respectively, and the breakings compared with the normal breakings of briquettes seven and twenty-eight days old kept in cold water.

The writer finds, in a general way, that the average of the breakings of hot water briquettes of pure cement, four days old, are nearly as high

as the normal seven-day breakings cold, and the hot water seven-day breakings of the pure cement are nearly as high as the normal twenty-eight-day breakings cold, where the cement is of good quality. Where the cement is poor and the pats show cracking and distortion, there is generally a remarkable falling off in the strength of the hot water briquettes from the above comparisons, and one system can therefore be used as a check on the other.

In Table No. 1, giving the hot water tests of twenty-three brands of cement, nine brands failed by their pats cracking or distorting when steamed and boiled; and with seven of the brands, whose pats thus failed, the briquettes subjected to the hot water test show a noticeable falling off as compared with the cold water tests. With the two brands whose pats failed by cracking, but whose hot water briquettes did not show any falling off as compared with the normal cold tests, it would probably be safe to use the cement.

Table No. 2 shows the behavior of each of the six pats made in connection with each of the twenty-three tests given in Table No. 1, and in the manner described on page 413.

In order to show the amount of free or uncombined lime necessary to produce swelling or cracking in the pats, the experiments given in Table No. 3 were made. From this it will be seen that a mixture of 1 per cent. of caustic lime with a normal cement is sufficient to produce cracking in pats Nos. 1 and 2, and that 5 per cent. is sufficient to produce cracking in pats Nos. 1, 2, 3 and 4, without this amount of free lime being shown at all by the cold water pat test, for which pat No. 5, described on page 413 in connection with the hot water tests, is made.

As a matter of fact, cold water pat tests, to indicate change of volume, as contained in the "Uniform System of Tests of Cement, recommended by the Committee of the American Society of Civil Engineers, June 21st, 1885," which require that they should be immersed as soon as they have set hard, are only of occasional use and rarely reject any cement except when free lime is present to the amount of at least 6 or 7 per cent. This test as prescribed by the "Amended German Rules," published in July, 1887 (which require that these pats, to determine change of volume, shall not be immersed until twenty-four hours after gauging), is not as good as that recommended by the American system, is of very little value in showing cements to be of inferior quality except when they are extremely bad; and as applied

TABLE No. 2.

CONDITION OF PATS AFTER BEING TREATED AS DESCRIBED ON PAGE 413 OF THIS PAPER.

No. of Test in Table 1.	PAT No. 1. Put in steam when made.	PAT No. 2. Put in steam when set.	PAT No. 3. Put in steam after twice the time of setting.	PAT No. 4. Put in steam after 24 hours.	PAT No. 5. Put in cold fresh water when set.	PAT. No. 6. Kept in moist air. Temp. 60°.	No. of Test in Table 1.	PAT No. 1. Put in steam when made.	PAT No. 2. Put in steam when set.	PAT No. 3. Put in steam after twice the time of setting.	PAT No. 4. Put in steam after 24 hours.	PAT No. 5. Put in fresh cold water when set.	PAT No. 6. Kept in moist air. Temp. 60°.
1	Good.	Good.	Good.	Good.	Good.	Good.	12	Good.	Good.	Good.	Good.	Good.	Good.
2	"	Bad.	....	"	"	"	13	"	"	"	"	"	"
3	"	Good.	Good.	"	"	"	14	Poor.	"	"	"	"	"
4	"	"	"	"	"	"	15	Bad.	Bad.	Bad.	Bad.	"	"
5	"	Bad.	"	"	"	"	16	"	"	"	"	"	"
6	"	"	"	"	"	"	17	"	"	"	"	"	"
7	"	Good.	"	"	"	"	18	"	"	"	"	"	"
8	"	Bad.	"	"	"	"	19	Good.	Good	Good.	Good.	"	"
9	"	Good.	"	"	"	"	20	....	"	"	"	"	"
10	"	"	"	"	"	"	21	Bad.	Bad.	Bad.	Bad.	"	"
11	"	"	"	"	"	"	22	Good.	Good.	Good.	Good.	"	"
							23	"	"	"	"	"	"

TABLE No. 3.

CONDITION OF PATS AFTER BEING MIXED WITH QUICKLIME, AND TREATED AS DESCRIBED ON PAGE 416 OF THIS PAPER.

No. of test in Table 1.	Per cent. of quicklime added to cement. Test No. 20.	PAT No. 1. Put in steam when made.	PAT No. 2. Put in steam when set.	PAT No. 3. Put in steam after twice the time of setting.	PAT No. 4. Put in steam after 24 hours.	PAT No. 5. Put in cold fresh water when set.	PAT No. 6. Kept in moist air. Temp. 60°.	No. of test in Table 1.	Per cent. of quicklime added to cement. Test No. 11.	PAT No. 1. Put in steam when made.	PAT No. 2. Put in steam when set.	PAT No. 3. Put in steam after twice the time of setting.	PAT No. 4. Put in steam after 24 hours.	PAT No. 5. Put in fresh cold water when set.	PAT No. 6. Kept in moist air. Temp. 60°.
20	1	A trifle bad.	A trifle bad.	Good.	Good.	Good.	Good.	11	1	Poor.	Poor.	Good.	Good.	Good.	Good.
20	2	Bad.	Bad.	"	"	"	"	11	2	"	Bad.	Bad.	Bad.	"	"
20	3	"	"	Bad.	"	"	"	11	3	Bad.	"	"	"	"	"
20	4	"	"	"	Bad.	"	"	11	4	"	"	"	"	"	"
20	5	"	"	"	"	"	"	11	5	"	"	"	"	"	"
20	6	"	"	"	"	Bad.	"	11	10	"	"	"	"	Bad.	"
20	7	"	"	"	"	"	"								
20	8	"	"	"	"	"	"								
20	9	"	"	"	"	"	"								
20	10	"	"	"	"	"	"								

The above pats were made from cement used in making Test Nos. 11 and 20, given in Table No. 1. They were mixed with quicklime just calcined, which contained nearly all pure lime.

to the Portland cements of commerce it hardly ever rejects any of them, although it is known that many brands of cement in the market are objectionable in quality, and that wide variations occur from time to time even in the better class of cements from their being imperfectly made.

It may be urged that while 5 per cent. of caustic lime is sufficient to disintegrate a neat cement pat when steamed and boiled, it might not affect the mortar used in actual work. This proposition is very hard to prove or disprove, except by a series of tests extending over very long periods of time, inasmuch as the slaking action in free lime contained in poor Portland cement mortar generally takes several months and often years before giving trouble.

In deciding upon the value of the hot water test, it is necessary to establish its usefulness, partly by analogy and partly by direct proof. Thus by direct proof and experiment we know by submitting cements of different qualities and composition to this test, that it ranges them somewhat in the following order: Cements of good quality, of normal composition and properly burnt, give satisfactory results. Cements of poor quality, especially those containing an excess of free lime, whether because of too large a proportion of chalk, imperfect mixture of the raw materials, or improper burning, cannot pass this test.

We also know that a cement containing 2 or 3 per cent. of caustic lime (added to a normal Portland cement) gives the following results: briquettes immersed in hot water promptly disintegrate, in sea water they decompose very quickly; the briquettes placed in air are reduced little by little to powder. Those which are kept in fresh water at the ordinary temperature are, on the contrary, not affected, and do not increase their volume. By analogy, we have a right to employ heat in developing quickly objectionable qualities in cement, on the theory that as it is found so useful in aiding chemical action, it is not unreasonable to suppose that a high temperature may show in a very short time the faults that would take months or years to produce failure in the work. Especially is this reasoning plausible when we know that good cements will stand this test and bad cements will not. By good cement is meant cements of the normal mixture in the raw materials, proper burning and grinding, and those giving a good Portland cement analysis after being manufactured, and which do not swell or crack in the work. From this the writer thinks it can be safely said

that cements failing to pass this hot water test should be used with extreme caution in important work.

The above-described hot tests are intended for cement to be used under water, and as many excellent hydraulic cements decompose when placed entirely in the air, the writer would recommend that where Portland cement is to be used exclusively in the air, it should be required to pass a hot dry test, instead of, or in addition to, steaming or boiling. For a long time the writer has been trying to find some ready method of chemical analysis by which the amount of free lime in Portland cement could be determined approximately, but at the same time with some reliability, but has not yet obtained anything satisfactory. Finding the amount of free or uncombined lime, by determining the carbonic acid and then calculating the amount of free lime present as carbonate of lime is perfectly unreliable, although it is given as the proper method by some authorities on cement. By this method, if the sample is treated just as it comes from the barrel, it must be on the theory that the free lime in the barrel has been first hydrated and then carbonated, two very slow processes which might take years to accomplish in a tight barrel; and if the sample itself is artificially hydrated and carbonated, we do not get a correct idea of the cement as it is contained in the barrel and is to be used on the work, even if the result were correct in determining the free lime present, which is doubtful.

Fresenius* says in relation to the determination of the uncombined lime "to effect the separation of the caustic or carbonated lime in hydraulic limes, from the silicates, Deville (*Compte. Rend.*, 37.1001, *Journal "F" prakt. chem.*, 62.81) proposed to boil with solution of ammonium nitrate, which he stated would dissolve the caustic lime and carbonate of lime, without exercising a decomposing action on the silicates."

Grunning (*Journal "F" prakt. chem.*, 62.318) found, however, that by this process the double silicates of aluminum and calcium are more or less decomposed, with a separation of silicic acid. As yet no method is known by which the object here stated can be accomplished with absolute accuracy; the best way perhaps is treating the sample with dilute acetic acid; C. Knausz (*Gewerbeblatt aus Würtenberg*, 1855, No. 4, *Chem. Centralblatt*, 1855, 244) recommends hydrochloric acid.

The following very roughly approximate method is herewith sug-

---

* Fresenius' "Chemical Analysis," translated by O. D. Allen and S. W. Johnson.

gested in determining the amount of free or uncombined lime in Portland cement. Take 1 gramme of cement powder and mix it with 40 cubic centimeters of boiled cold distilled water in a stoppered flask. Shake frequently for one hour and then filter off 20 cubic centimeters of this solution and determine the amount of lime in the same by the usual methods.

While it is well known that pure water dissolves the silicates and aluminates of lime in Portland cement, preferably the latter, the action does not begin until the solution contains less than 0.62 gramme of lime per litre.

In the above method, if the cement contained only 2½ per cent. of free lime, the above solution should contain at least 0.62 gramme of lime per litre, and consequently the silicates and aluminates of lime would just begin to be dissolved by the water.

There being very little assistance to be derived from chemical analysis, in determining the amount of free lime in Portland cement, which is shown so quickly and sharply by the hot water tests, attention has been directed to the behavior of this cement in sea water; and it has been suggested that instead of using the hot tests to which some object on account of their violence, the pats of pure cement to show change of volume should be immersed in sea water of normal temperature, where a cement containing 1 to 2 per cent. of uncombined lime generally shows cracking in a very little time.

The action of sea water and of the hot tests upon Portland cement have some points of resemblance. With the hot tests, pats of pure cement show the presence of free lime more readily than the sand mixtures, as do the pats and briquettes of pure cement containing free lime immersed in sea water, which after a lapse of a certain time invariably show signs of failure; while the briquettes of sand mortar are not affected as much in the same time. These facts only emphasize the importance of making the hot test, both with the pure and sand mortars.

Another test for the determination of free lime, is that of the increase of temperature when water is added, and if this is very carefully conducted, so as to take into consideration the amount of heat lost in radiation, some quick results can be obtained.

For instance, in a set of experiments made by the writer with an extremely sensitive thermometer, graduated to tenths of degrees (Cen-

tigrade) cements containing very small amounts of free lime raised their temperature 0.20 degrees Centigrade, when 50 cubic centimeters of water were added to 50 grams of cement, both exactly of the same temperature at the moment of the experiment. With cements containing an objectionable amount of free lime a rise of temperature of 0.50 to 0.55 degrees (Centigrade) was found under exactly the same circumstances. The small difference in the temperature between adding water to cements of poor and good quality, makes the experiment a very delicate one.

The following extract translated from M. E. Candlot's work on cement, above referred to, is very pertinent to the subject of this article. He says, with reference to the tests with hot water: "Some years ago, in Germany, they extolled the tests with boiling water, to obtain in a short time resistances as high as those shown by mortars immersed in the water at the ordinary temperature after several months or even several years. It was said that the maximum resistance that pure cement is capable of reaching was obtained at the end of seven or twenty-eight days' immersion in boiling water. This claim was not justified and all of the tests that have been made with a view of stating the resistance of pure cement mortars immersed in hot water have not given conclusive results. The hot-water test may, however, give a very useful piece of information in showing the presence of free lime in a lime or cement. In fact, a cement to which is added one-half of 1 per cent. only of azotate of lime highly calcined, swells when it is immersed some time in boiling water of from 70 to 80 degrees (Centigrade). A cement which resists well this test is certainly free from uncombined lime. It is therefore a precious gift and easy to obtain. But if the mortar swells or presents fissures, should we conclude that the cement is certainly bad? A cement of normal composition, not containing lime in excess, but whose burning has not been pushed to vitrification, swells enormously when it is immersed in hot water. However, we have seen similar cements give excellent results, completely different from those that have been given of cements that contain an excess of lime. The latter show fissures more or less deep when they are immersed in sea water, the mortars in the air give feeble resistances, and are sometimes reduced to powder. With cements imperfectly burned, but well proportioned and homogeneous, nothing similar is observed. These mortars are permanent in sea water, show

no signs of alteration and the test pieces kept in the air give results entirely satisfactory. The test of pure cement in hot water would condemn such cements as containing an excess of lime."

"The manner of directing the tests which has been described by M. Le Chatelier in connection with the experiments made by M. Deval (*Bulletin de la Société D'Encouragement Pour L'Industrie Nationale*, Août, 1890) seems preferable to us, and capable of giving the most useful information. It consists of placing briquettes of one to three mortar, made in the ordinary manner in water kept at 80 degrees (Centigrade). The briquettes are broken at the end of three and seven days, and the results are compared with those given after seven and twenty-eight days with briquettes of the same cement kept in water of the ordinary temperature. Cements containing free lime give, in hot water, resistances more feeble than in cold. Cements of good quality give resistances at least equal and nearly always greater in hot water than in cold. Cements well proportioned and homogeneous, but not having obtained the maximum burning, give with this test satisfactory results."

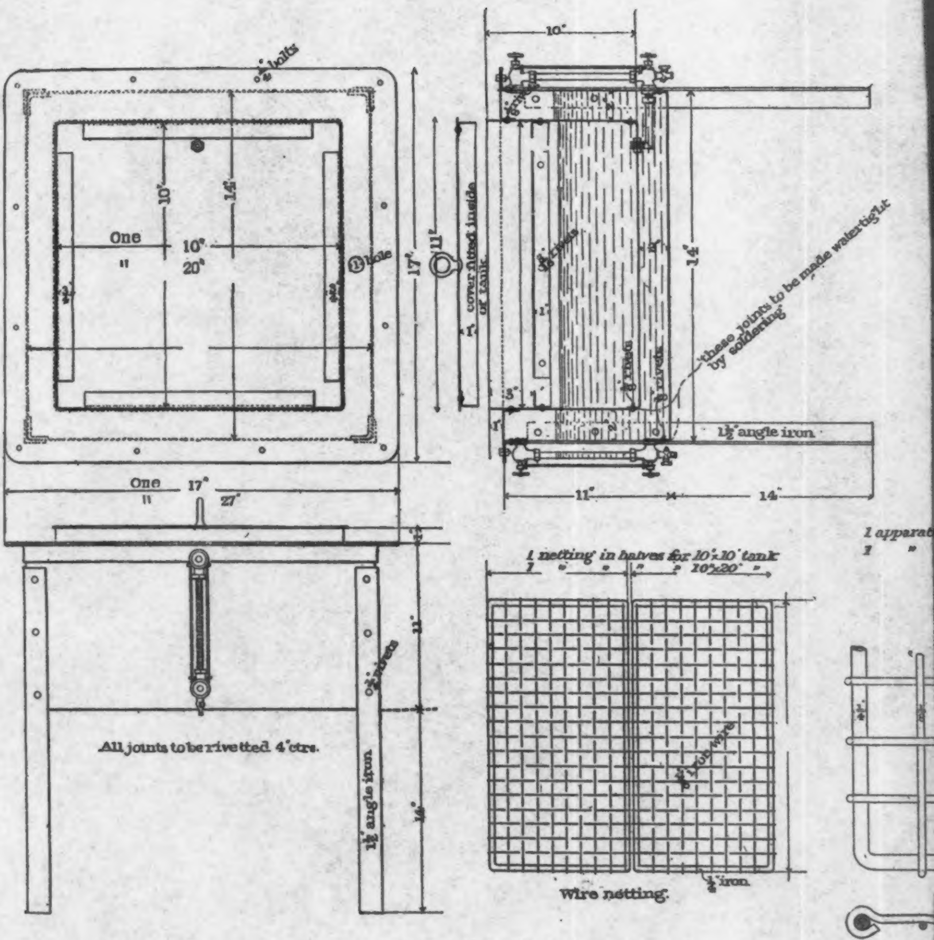
In conclusion, the following extract has been translated from a report made by M. Le Chatelier, in the name of the chemical committee, upon a paper by M. Deval relative to hot water of cements and hydraulic limes (*Bulletin de la Société D'Encouragement*, Paris, Août, 1890): "The summary of the experiments reported here show that hydraulic products of good quality, and of normal manufacture, are ranked practically in the same order by tests either hot or cold, the deviations not being greater than should result from errors of experience. Products containing free lime, which in the cold tests have an initial hardening sufficiently rapid and which consequently are well graded (by the cold test), are at once rejected by the hot tests."

"Finally, products containing pozzuolanic material, slightly energetic, cinders or slag, gain notably in the classification by the hot test. In the cold test these constituents do not play any rôle in the initial hardening; they act simply as inert sand."

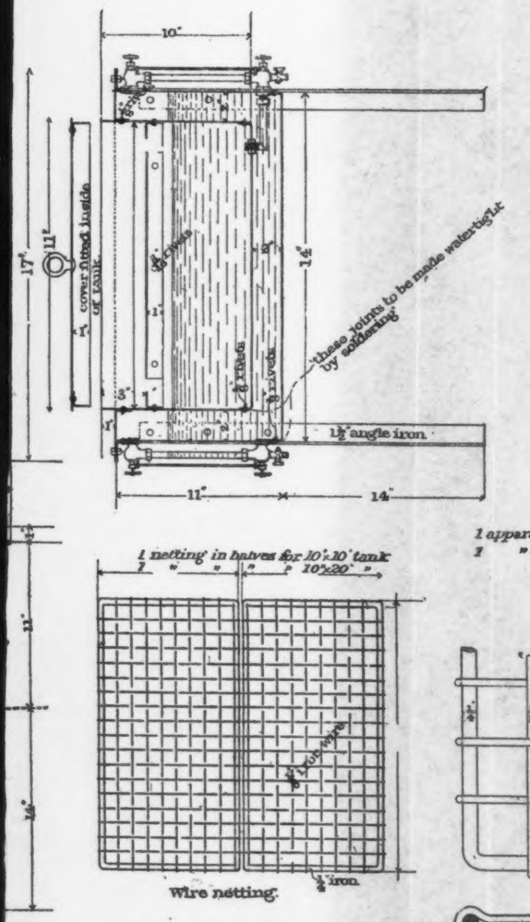
"The use of the hot tests presents an absorbing interest to all manufacturers who wish to follow closely their own productions, above all to the manufacturers of natural Portland and of hydraulic limes. The greatest danger that is met in these productions is the excess of free lime, which leads eventually, in the work, to the most deplorable deterioration, the masonry joints open and the tops of the

wall are raised. Hot tests lasting only a few days are sufficient to inform the manufacturer in a sure way upon the quality of the products he delivers to the consumer. The cold tests do not furnish any like information. The results would be the same with artificial Portland cement, but in this case the hot tests have less interest for the manufacturer, who, for the greater part of the time, knows in advance the quality that he will obtain, according to the care which he bestows upon the mixture of the raw materials and their burning."

"That manufacturers protest against all hot testing on reception, goes without saying. These tests establish a separation much too sharp between products of the first quality and of mediocrity, and render more difficult the sale of the latter, viz.: Artificial Portlands, poorly burnt; Natural Portlands with excess of lime, and hydraulic limes badly slaked, etc. They might accept these tests if carried on inside of their factories; they must refuse them when made outside. The consumers, from opposite motives, ought to extol this method of testing. The reason of their opposition springs from having for a long time employed cold tests, they are led naturally by the single fact of custom to consider them as approaching perfection. The hot tests and the cold tests do not agree. The one or the other must be wrong. This condemnation, it is necessary to say, is in part justified by the fact that Michaelis, the first promoter of the use of the hot water tests, had announced an absolute proportionality between the hot and cold resistances (tensile strengths). The incorrectness of this proposition was proved a long time ago by the experiments of the Calais Laboratory, and the new method of testing has been condemned upon this simple proposition. Now, it is precisely this want of proportionality that constitutes its merit. It permits the rejection of certain very objectionable products that derive their advantage from the cold test. It may, nevertheless, be a question whether it is advisable to actually change suddenly from the cold tests to the hot in the present conditions of receiving cement. The cement factories exist to-day under a régime which favors a material containing a small quantity of free lime; they are obliged to regulate their production in consequence; it is not possible from day to day to modify their condition of existence. But it would be desirable in the laboratories of the State, where tests on cement received are made, that they should make regular hot tests of resistance, by the side of cold tests of resistance."

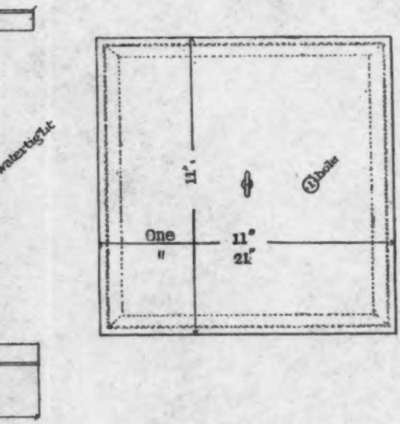


Steam apparatus
for testing briquettes.
May 12, 1891

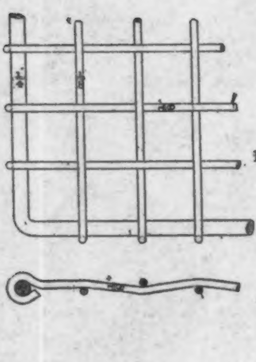


Steam apparatus
for testing briquettes.
May 12, 1891

PLATE LI.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 555.
MACLAY ON HOT TESTS FOR PORTLAND CEMENT.



1 apparatus with inner tank 10"×10"
1 " " " " 10"×20"
} All heights same for both



Half size view of netting.

"A consistent comparison could then be made, not in seeking if the correspondence between the resistances, hot and cold, is regular, which certainly it is not, in view of the tests reported here, but simply in studying more closely those cements in which these two modes of testing lead to results notably divergent. One should then recognize the condition of the production of these materials, and observe with more care the manner in which after a long time they behave in the work. In a few years one could fix upon a method of classification which would more nearly approach the truth."

"The tests of hot resistance should be made by immersing the briquettes in water of 80 degrees (Centigrade) twenty-four hours after gauging; for limes feebly hydraulic only, the immersion should only be made at the end of three days. The briquettes should be broken at the end of seven days after their immersion in hot water. Corresponding tests should be made in twenty-eight days in cold water. In all cases mortar of normal sand (one to three) should be used, strongly rammed according to the Boulogne specifications."

DISCUSSION.

Mr. R. W. Lesley.—I want to say very frankly, as is probably known to the Chair, that I am a manufacturer of Portland cement. As a manufacturer of cement, and merely an invited guest at this very interesting meeting, it is with considerable diffidence that I venture to raise some questions as to the value of the new test for Portland cement recommended by Captain Maclay. My only excuse is, that to-day, as in the earlier days of Portland cement, it is the interest of both manufacturers and engineers to secure the best results and to co-operate in prescribing such tests as will secure these ends and maintain and increase the confidence of the public in cement and cement construction.

When, in 1865, and again in 1871, in the formative days of the Portland cement industry, John Grant, the pioneer among engineers in the testing and use of Portland cement, read his results before the Institution of Civil Engineers, in London, the leading manufacturers in England, such as White, Francis, and others, were present and took part in the discussion, and aided in the formulation and determination of the proper standard of tests. So, too, in Germany, manufacturers

and Government engineers confer in coming to conclusions as to normal tests; while, in the preparation of the American Society's suggestions for specifications for cement, the services of the well-known manufacturer, Mr. Norton, were called in as a member of the Committee. With these precedents in mind, and with no wish but to conserve the best interests of engineers, consumers and manufacturers in the use of an article of the greatest importance, I venture to make a few remarks upon Captain Maclay's paper.

The object of Captain Maclay's paper is, as stated, "to show that the test generally employed heretofore, to determine the change of volume in Portland cement, is of very little practical value, and should be changed to a steam and hot water test." In accordance with this recommendation of his, he has, already, in the specification of the Department of Docks, dated May 1st, 1891, required substantially the elements already stated in his paper as to hot water and steam tests. In paragraphs 10 and 11 of that specification, it is required that, after the hot water test, "the cakes of cement should still adhere to the glass"; and in Article 17 of that specification, it is stated that "the neat cement, when set, must show no distortions or cracks, and must comply with the requirements of paragraphs 9, 10 and 11, in the memoranda of testing already quoted. Presumably, therefore, should the American Society determine that the new test, such as recommended by Captain Maclay, is necessary, a specification somewhat similar to that mentioned would be suggested by the Society.

In order to test this specification and its utility, it behooves us, as manufacturers, and you, as engineers, to consider well and to go slow. In the first place, in reading over Captain Maclay's very interesting paper, it will be noted, that out of twenty-three brands of Portland cement examined, nine failed to stand the requirements of his specification—a proportion out of all reason, with present methods of manufacture. It will be, moreover, noted in all that he has written in his paper, that there is not one word stated or explained as to what results were obtained at long periods, or, in fact, at any periods over seven and twenty-eight days, with the pats or with the cements referred to, either in the laboratory or in the work. There is not a word as to the chemical composition of the cements accepted or rejected, nor as to their fineness, nor as to the proportions of water used in making the pats; all of which facts, as I will show, have a most material bearing on the question at issue. In other words, there is nothing to show that, in practical results, the accepted cements were good, or the rejected cements bad, nor is there anything as to any of the material elements of the test, outside of tensile strength and boiling.

Now, in coming to the recommendation of so serious a change in the modes of testing as this, it would seem to be a prime necessity that all the practical results in the laboratory, as well as in the work,

should be fully set forth before making such a recommendation. Upon this point, I propose to touch more fully, when I give some actual results with cements, tested under the methods described by Captain Maclay, at long periods. But upon the question of standards, the change of standards, the establishment of standards, it is certainly necessary to consider whence the standards are derived. The authorities quoted in support of Captain Maclay's paper are two French writers, Candlot and Le Chatelier, and not a single authority from Germany, England, Belgium or the United States. Now, in considering the weight of authorities, it would seem to be first necessary to consider the history of the industry in the place whence the authority comes. Taking the history of Portland cement, it is well known that it originated in England. To-day, England manufactures from 7 000 000 to 8 000 000 barrels per annum; from England, the industry went to Belgium, where nearly a million barrels per annum are manufactured, and to Germany, where, it is claimed, from 9 000 000 to 10 000 000 barrels per annum are made. France, though the earliest known in the chemical investigation of cement, was almost the latest of the great nations to make Portland cement, and to-day her product is barely a million and a half barrels. Now, French engineers use largely lime of Tiel, which is hydraulic lime (see Le Chatelier), and their experience is largely confined to materials of this kind; though, of late years, the use of Portland cement has increased.

In considering the question of authority, therefore, taking the standard specification of the German Society of 1887, which requires no boiling test, but an exposure in thin pats to water for twenty-eight days, after having been first exposed in air for twenty-four hours; taking the English specifications, such as are given by Reid in his work on concrete, page 50, which requires "that the pats shall be left in water for six days, and shall neither crack nor fracture"; Newman, who prescribes practically the same requirements, and John Grant, who even did not go so far as to make pats; and taking the American Society's suggested specifications, formulated by the very leading minds on this subject in the country, which require two pats of neat cement, "one of which is to be put in water and the other in air, and which are examined from time to time for checking," it will be seen that in none of the countries where Portland cement is best known and most largely used, is there any specification whatever requiring boiling, steam or hot water tests for cement.

After considering the weight of the authority, and considering the fact that possibly two French writers such as Candlot and Le Chatelier might outweigh the consensus of authority that has gone before; let us look at the results of the use of Portland cement by French engineers, and consider for a moment the very small quantity of Portland

cement which has been made and used in France, as against the use of the hundreds of millions of barrels of Portland cement made and used in Germany, England, Belgium and the United States, within the last forty years with the few adverse results of its use. Even Candlot himself is in doubt as to his own reliability and that of the French writers as authority, because on page 129, he says: "In the German manufactories the testing is never done at the factory; the cement once arrived at destination, is tested conformably to the rules established with great care by the manufacturers of cement and the Government Administration; the use of Portland cement, which is much larger in Germany than in France, has never given rise to disasters." Possibly M. Candlot had in his mind at that time, the large bridge at St. Nazaire, where cement passed by engineers of the French Government, disintegrated after eight years, and caused the fall of the bridge (see *Building News*, London, May, 1887). But even in France, whence these two eminent authorities cited by Captain Maclay come, the very latest specifications by the French Government for the use of engineers doing work, both under sea water and in the air, requires no boiling, no hot water test, no clinging to the glass, none of the things referred to by the writers in question. It requires practically the same tests for checking as those of the American Society, with the single difference that where the cement is to be used under salt water (Cahier Guillain) that the pats shall be placed in salt water, with a temperature between 60 and 70 degrees Fahr. So much for the question as to the weight of authority for changing the present very carefully and skillfully drawn suggested specifications of the American Society, and for introducing into them new elements of doubt and of uncertainty.

Now, upon the question of boiling of cement, how far do these gentlemen who have amused themselves in this way in their laboratorical cook-shop agree as to how the cement shall be tested; for boiling cement is no new thing. We have Mr. Tetmajer and his followers of Germany and Switzerland, who propose to put the pat in cold water, and raise the same to a temperature of boiling, and examine the pat after four to six hours; who propose also to place it in water twenty-four hours after it has set, and to raise the temperature to 120 degrees Centigrade (Tetmajer, Account of Sub-Committee No. 12, Zurich, 1889). We have also another amusing little way of theirs of making a small ball of cement and, before it has even time to set, holding it over the mouth of a Bunsen burner until it is red hot (Candlot, page 146). We also have another mode of torturing cement, by putting it in a closed chamber and giving it a steam pressure of 10 to 15 atmospheres (Proceeding, German Cement Manufacturers, 1891). These are only a few of the interesting little frills of our German and Swiss friends interested in accelerated tests, against whose amusement the

present German normal test has been held unimpaired for thirty years by the Government and manufacturers, though repeatedly attacked by these enthusiasts.

We have Mr. Faija, of London, from whose book, published in 1890, much of Captain Maclay's very interesting methods have been derived. Mr. Faija thinks a temperature of 117 degrees Fahr. about right; Captain Maclay thinks 200 degrees Fahr. about right; M. Le Chatelier prefers 80 degrees Centigrade, while M. Candlot seems to be willing to agree with any and everybody as to temperatures, but expresses, as does M. Le Chatelier (*Bulletin de la Société D'Encouragement*, etc., August, 1890), very great doubt as to the utilization of this test at all among engineers and consumers, but relegates it with M. Le Chatelier to the manufacturer as something very interesting for him to amuse himself with (Candlot, page 149) and properly so, because, says Professor Schuman, the German cement expert (Proceeding, German Cement Manufacturers, 1891) "until now, neither the degree of temperature nor the duration of influence has been fixed," but, even if they were fixed, "it would be absolutely necessary to have always an exact temperature, and this is only possible when the gas-fittings and the contrivance for regulating the heat are always properly disposed of." Mr. Faija in a recent discussion states that he devised some years ago an apparatus for determining the soundness of cement, in which he treated the cement at a temperature of 110 degrees. He had lately made hot water tests of cement, and found that he could blow any cement to pieces if it was subject to the specified temperature of 180 degrees long enough, and therefore boiling was not indicative of bad or good cement. A specification recently sent him stated that the cement "must be gauged and placed in the testing moulds on plate glass. At the expiration of three hours, the casts were to be taken out of the moulds, and exposed to the air for twenty-four hours, after which they were to be immersed in boiling water and kept at boiling point twenty-four hours, during which time they must not show any signs of disintegration." He admitted that he was unable to advise the manufacturer what to do to comply with these terms. For ordinary purposes, they only had to find out whether it was sound or not; and that could be done without boiling, which had nothing to do with the constructive value of the material.

To say nothing of temperatures, therefore, nor of the time the pats are to be boiled, all of which is in doubt, the authorities on this proposed boiling test do not even agree in the simple question as to whether the pats are to adhere to the glass plates or not, for Tetmajer, in the paper above quoted, says that this is an essential of the boiling test, while Captain Maclay in the paper before us, says, "that this test is not insisted on," and yet, in the Dock Department Specification already quoted from, it is one of the essentials stated.

Mr. Rudolph Dyckerhoff, in a paper read before the German Cement Manufacturers, at their meeting in February, 1891, pages 60 to 87, lays down the fact that the finer the cement, the fatter it is, the more water it needs and the more it shrinks. Now, none of these gentlemen who have prescribed this boiling test seem to have considered the question as to the amount of water that is to be mixed with the cement out of which the pats are made. None of them seem to have considered that in these days of fine grinding, that fact may be a material one in the testing of their cement. It may shrink and leave the glass, owing to the fineness of the grinding or the lack of water in mixing. So that, on the whole, so far as the boiling of cement is concerned, it may be that in making any change in the specification we are going a little away from the common sense view of cement testing; from Faija, who says, "the shorter the specification, the better"; from the Germans, who have one of the shortest of specifications; from the American Society's suggested specification, which is intelligence itself; and from the specification of the United States Government, H. Clifford Richardson, Government Chemist; and are getting into that realm of uncertainty where the engineer and the cement manufacturer must stand hand in hand with Centigrade, Fahr. and "sensitive" thermometers around and about them, and all the elements of a college for the education of very young men, within easy reach.

John Newman, whose book on concrete, London, 1887, is one of the most sensible, so far as practical results are concerned, sums up in three pages, 15, 16 and 17, out of the difficulties which affect the testing of cement, only forty reasons and conditions that may adversely or favorably affect cement tested under the ordinary English rules. Adopt these new methods of testing and there will be four hundred conditions, and colleges will have to provide special courses for the education of cement testers. No one can object to proper, accurate and certain tests, much less the manufacturer than the consumer or engineer, but we ought all of us to insist on, at least, that degree of accuracy in a boiling test that will tell us whether we are to have our pats "hard," "soft," or "medium" boiled, and whether the minute glass governing the period of their immersion is to run by the minute, hour or day.

Now, leaving the reasons already adduced, which would seem to carry some weight, let us see what are the practical results of the boiling test.

Referring to the proceedings of the German Manufacturers' Association, acting in conjunction with the Administration of German Public Works, held in 1891 (Protokol des Vereins Deutscher Cement Fabrikanten, Berlin, 1891), it was unanimously resolved to "adhere to the normal test for Portland cement"; that "there are no experiments per-

mitting an essential objection to the certainty of the normal tests, and so while the accelerated tests for constancy of volume may be suitable as means for manufacturers to determine the character of their cements, they are not appropriate for the consumer to obtain a sound opinion on the constancy of volume." This conclusion was not arrived at hap-hazard, but it, as well as the subsequent results of other experiments that have been made on this question, are predicated, not upon what a number of little pats of cement did in hot water, but upon what cements that did not stand the boiling tests did do in actual work for a period of years. These tests, which form the body of a report made to that society at that time by a committee appointed for that purpose, were as follows:

"Whenever we found in the market a cement which could not stand the accelerated tests, we made cakes of it.

"The following observations in the methods of hardening have been made. Pats or cakes were made and placed:

"1. Only in the water.

"2. Twenty-four hours kept wet, but then hardened in the open air.

"3. Three days in water, and then hardened in the open air.

"4. Seven days in water, and then hardened in the open air; and finally,

"5. Twenty-eight days in water, and then hardened in the open air.

"At the same time, the following experiments were made to ascertain the strength of mortar, consisting of one part of cement and three parts of ordinary sand used by bricklayers:

"1. Only hardened in water.

"2. Hardened in the air, in a room or out of doors.

"3. Twenty-four hours in a wet place, then open in a room or hardened out of doors.

"4. Three days in water, then hardened in the air, in a room or out of doors.

"5. Seven days in water, then hardened in the air, in a room or out of doors."

"This series of experiments could, however, not be made with all the cements, because there was not always enough of material, but always the experiment was "three days water, then air" (either hardened in a room or out of doors), and this experiment is of great consequence.

"The experiments proved, that the cake test in water (normal test) was faultlessly endured. I must once more emphasize this, and reiterate that only such cements were made use of for experiments as had not endured the accelerated tests. Cements that could not stand the regular normal test of the German specification, we were obliged, of course, to exclude. Those cakes which were kept moist only twenty-four hours, all became by and by more or less soft. On the other hand, cakes that had been in the water for three days all kept well, and, of course, also those which had been in water seven or twenty-eight days. Keeping them in water three days had also prevented the softening of the cement. The tests showing the tensile strength against all expectation turned out well. It was thought that such briquettes which were directly exposed to the open air, or remained

in a moist form for but twenty-four hours, would show a bad hardness. But this has not been the case; moreover, the series of tests have shown a regular increase of hardness, only the briquettes which had been in the water for three or seven days had, of course, a greater strength than those which were not in the water. I don't wish to put before you all the numbers, but they may be disposed of by any one who is interested in them. I must not forget to mention, however, that cements which in the accelerated tests showed great strains or a growing crookedness of the cakes, were in tests for strength not worse than cements which in the tests seemed to be but little attacked. * * * It has also occurred that cakes of a cement which had not endured the hot test have been preserved safe and sound up to date, over three years.

"These tests therefore are in contradistinction to the results of the accelerated tests for finding out the constancy of volume, and show positively that the accelerated tests may not serve as a standard in deciding about the avoidability of a cement in case the accelerated tests should not have been endured."

In addition to these practical tests, Prof. E. J. De Smedt, for many years Government Chemist at Washington, and a leading authority and writer on cement, also conducted a series of tests in the same direction, which will be interesting to us in this country where millions of barrels of natural cement are used with entire confidence and success, and which cements, if tested under the boiling test, would all be condemned as unfit for use. In these series of tests, the following results were obtained:

Prof. De Smedt says in his report: "The purpose aimed at by this boiling test seems specially to be to determine the existence of any free lime in the cement, which, when used as mortar in hydraulic concrete or any other work, might have a final detrimental effect on it. Several other well-known methods of determining free caustic lime (CaO) in hydraulic cement are already at our disposal, whereby the safety or unsafety of a cement can be pronounced without any difficulty and without risking the condemnation of good, reliable cements.

"If sound theories, however, and intelligent comparative experiments (of which no mention is made) form the basis of this proposed boiling-water test, it will be received gratefully; but no mistake of any sort can be permitted on this line, for the risk is too great of compromising thereby the interests of others.

"I therefore take the liberty to submit for discussion, the following remarks and queries:

"The first question of which we are reminded is this: Does this 'boiling-water test' give positive evidence of the presence of caustic lime (CaO) in hydraulic cement? In order to answer this inquiry it became necessary to make tests with Portland cements and with the natural light-burned cements, such as Rosendale and others. Fifteen tests of natural light-burned cements, including all of the standard Rosendale, Potomac and Lehigh cements, were made; two of these stood the boiling in water for two hours, at the expiration of which time they left the glass; in all of the other thirteen tests, the cement cracked and crumbled during the boiling of the water. These thirteen samples, as proved by tests, contained no caustic lime, and were slow-setting cements, while the two samples first referred to as standing the boiling in water better than the other thirteen, were quick-setting

cements, and contained more lime soluble in water than the latter. Thus, it seems that this test does not confirm the idea of its value as a test for determining the existence of caustic lime (CaO) in this class of hydraulic cements. All the samples of the natural cements tested by the boiling-water process were of the natural cements known in the market as having given excellent results as hydraulic cements in every kind of construction.

"Tests for caustic lime were made by the ordinary well-known process on one dozen different brands of Portland cement.

"One only was found to contain caustic lime; the other eleven were considered free from caustic lime and were classified as safe and reliable cements. All of the twelve samples were then submitted to the boiling-water test. After a few hours of boiling, all except two left the glass, and none cracked or crumbled during the operation. Yet, with the exception of the cement first referred to, which was by the ordinarily adopted tests found to contain caustic lime, I consider all the rest of these cements good, reliable Portland cements.

"These experiments, therefore, thus far seem to prove that the water-boiling test is not fair and reliable as to the finding of caustic lime in hydraulic cements, but it is rather a deceptive one, whereby good, reliable cements may be classified as unsuitable and dangerous. This is unfair and discouraging to honest manufacturers.

"Let us examine whether this boiling-water test is not apt to produce undesired and detrimental effects on first-class hydraulic cements during the setting stage. It is a well-known fact that hydraulic cement gauged by hot water will set quickly, giving a high tensile strength in a short time, but will finally return to a low and poor result. Pure aluminate of lime heated at 200 degrees Fahr., bursts and dissolves into powder while setting. At an ordinary temperature it sets extremely hard and is considered one of the principal elements in the setting and hardening of hydraulic cements with water. All good Portland cements contain free lime soluble in water, which is anhydrous (CaO) immediately after calcination. It absorbs water from the moist atmosphere, whereby the caustic lime is converted into hydrate of lime.

"The setting and hardening of hydraulic cements with water seems to be the result of these different chemical actions; 1st, the hydrating of the aluminate of lime, and 2d, the re-action of the free hydrated lime on silicates of lime, simple or multiple, existing in all hydraulic cements, and acting like pozzuolana.

"The calcination of lime with clay produces no good hydraulic cement, unless the proportion of clay to that of lime is such as to form, 1st. Aluminate of lime, represented by one of the following formulas—

"Al O CaO; Al O 2 CaO; Al O 3 CaO.

"2d. Silicate of lime, simple or multiple, having a pozzuolana quality. 3d. Free lime acting on the pozzuolana silicates, and forming silicates approaching the following formula—

"SiO 2 CaO 3 HO; SiO 3 CaO 4 HO.

"The above facts and theories appear to demonstrate that it is advisable before the adoption thereof, that the boiling-water test should be submitted to critical comparative experiments, and that not until good scientific and practical proofs have been established should its use as a test be authorized."

In addition to these practical results, I have brought with me some briquettes of a cement that about a year ago was tested at our works,

and stood the boiling test. It is a cement known for this peculiar element of its character, and for uniformly standing this particular test. All of the briquettes which I have here, both neat and with sand, checked, disintegrated and swelled; one of the briquettes having an inch breaking section was put upon the mould in which it had been made, and showed a swelling of between one-eighth and one-fourth of an inch. Furthermore, as an illustration in briquettes of the result of depending upon the boiling test, a very interesting series of figures upon slag cement were made also at our works, under the most careful conditions, the object being to determine the value of the slag cement patents which our company was at that time about to purchase.

The slag cement stood the boiling test, and gave the following results at short periods. Briquettes made July 11th, 1888.

3 days in water, neat,	258 lbs.		
7 " " " "	360 "	1 sand, 1 cement, by volume	420 lbs.
19 " " " "	330 "	1 sand, 1 cement, by volume	382 lbs.
21 " " " "	432 "		

And all the indications were in favor of the value of the cement. At the end of three and a half years, namely, on January 20th, 1892, the following breaks were made:

Three and a half years in water, neat.................. 221 pounds.
" " " in water, neat...................280 "
" " " in water, 1 sand, 1 cement, by vol.510 "
" " " in water, 2 sand, 1 cement, by vol..400 "
" " " in air, neat, broke while putting on clamp.
" " " in air, 2 sand to 1 cement, by vol. . 100 pounds.

All of the sand briquettes kept in air are disintegrating and falling away. These figures are interesting, not only from the fact that they show the boiling test to be unreliable in judging cements of this character, which are now largely coming on the market, but also from the fact that these are practically the longest time tests that have been made so far upon slag cement. So far as the practical work with this cement is concerned, an expert sent to England to report upon a dock built with the same slag cement tested above, reported that the work had all crumbled and gone to pieces.

From these three examples of practical results which, as already stated, are of the cement tested after having endured, or failed to endure, the accelerated test of boiling, steaming, etc., much stronger arguments can be made for the adherence to the established standards than can be made from the paper which Captain Maclay has presented this evening in favor of any change. The sum and substance of the chemical portion of his paper is, that because certain cements fall apart when subjected to the boiling test, these cements are not good. There are no

actual results of the use of the cement, and no long time tests on them in air or in water given, but it is stated that because a certain cement, boiled by itself, does not fall apart, and the same cement falls apart when quicklime is added to it, that, therefore, a second cement which falls apart in the boiling test, does so by reason of the fact of an excess of free lime, and this without any analysis of the two cements thus tested. This mode of reasoning is not conclusive in a chemical question of this kind. Suppose, for instance, some other material had been added to the first cement other than quicklime, and had caused that particular cement to fall apart. There are a number of chemicals, such as oxide of barium, magnesia, an excess of clay, any one of which would cause the result stated From an experiment, therefore, conducted with any of these chemicals, the same argument might be made, that when a second cement, to which the chemical had not been added, falls apart, that the falling apart is due either to oxide of barium or magnesia; so that even as determining that a cement which falls apart in boiling does so by reason of an excess of free lime, the boiling test fails.

Let us see whether there is not a better reason chemically than that given by Captain Maclay. Is it not possible that the checking of pats under the boiling test is due to the aluminate of lime in the cement? This is a requisite element in all Portland, and one without danger to the cement. Is it not possible that when we know, as we do from the authorities Chattonay, Rivart, Fremy, De Smedt, Deval and others, that the aluminate of lime is the first thing to cause the setting of cement, and that, therefore, when this element is present in considerable quantities, as it must be in Portland cement, the action of setting is disturbed almost immediately by subjecting the pats to the torture of boiling, steam, etc., that this has something to do with the matter? Is it not possible that when we consider the property of aluminate of lime, which, when "exposed to a temperature of 200 degrees, bursts," according to De Smedt, that this element disturbed in the moment its chemical action has begun by exposure to high heat at an improper time, swells and expands, and it, and not free lime, is the cause of expansion?

In view of the fact that among the authorities on cement who have followed up the line of investigation opened up by these writers who have made a study of aluminate of lime, a new field of discovery is being laid open, and in view of the fact that, in this direction, new aluminate of lime cements are likely to come in a measure to take the place of the cheap slag cements now made; is it not possible that by adopting a boiling test which would inevitably destroy a pure aluminate of lime cement, and one which, under practical every-day conditions of work, it would never meet, a new and large field of discovery would at once be closed up? Though no Portland cements of this kind are made in this country, the French Ciment de Vassy is an

illustration of the type referred to. In view, therefore, of the possibility of the results of this investigation, and in view of all the doubt that must certainly exist in the mind of every scientific and careful man who has heard both sides of this question of accelerated tests to-night, it would seem that the matter is one that for the present at least should be left open pending further investigation and further light.

J. J. R. Croes, M. Am. Soc. C. E.—The remarks made by Mr. Lesley are open to objection in two respects. Mention was made of the "Standard Tests" of the American Society of Civil Engineers. The fact is that the Society has no standard tests of any material. It has always been contrary to the policy of the Society to promulgate any methods of testing materials as the best or only methods permissible. When the manufacture of cement or iron or steel or any material has progressed to such a point that uniformity in methods of testing is desirable, a committee of the Society is very properly appointed to investigate these methods and recommend suggestions which are in the line of advance, but never has the Society as a body formulated any standard for either manufacture or testing of any material. Such practice would be contrary to science and civil engineering, and when a manufacturer comes to us and says that the tests he employs are good enough and he wishes no change made, it particularly becomes the duty of the Society and its members to investigate the subject in order to find out whether there should not be something more done about it and whether better results cannot be obtained. The ground taken by Mr. Lesley was well described in the extract from a paper by Le Chatelier quoted by Mr. Maclay: "That manufacturers protest against all hot testing goes without saying. The reason of their opposition springs from having for a long time employed cold tests and they are led to consider them as approaching perfection."

The paper of Mr. Maclay is just a step in the right direction. It is a record of what he has done and what he has found by his methods. Very interesting results are obtained, and he states very frankly in some respects he does not think that these are perfect, but as far as he has gone he thinks that certain conditions should be maintained in the testing of cements, conditions not in the old creeds; that the manufacturers have got to know how to comply with them in new directions, and developing new results, and it is to be hoped with advantage to the manufacturer of cement.

The writer of the paper does not contend that this test is absolutely infallible, but he says on page 415 that he does think that the "failure of a cement to pass this test throws grave suspicion on its quality and fully justifies its rejection, especially when it is corroborated by the low tensile strength of the briquettes with neat cement and two parts of sand steamed first and then kept in hot water of the same temperature as the pats." That seems to me to be as far as the paper goes; that

the results obtained by the experimenter led him to certain conclusions, which he states, and they are exceedingly interesting, and they are greatly to be commended and not to be attacked, particularly by manufacturers who are interested in preserving certain methods only.

As I understand Mr. Maclay's paper, it does not refer at all to natural cements, such as the Rosendale. If any new or additional methods of determining the values of Rosendale cements can be suggested by any member, they will undoubtedly be gratefully received and investigated.

Mr. Lesley.—I want to say to Mr. Croes that he is perfectly right in implying that I am here as a matter of business, but I want to say that in the discussion and examination of a scientific investigation such as this, that it is certainly business to look at both sides of the subject and to draw attention to that which seems to lack in accuracy and precision. It may not be technical, and it may not be scientific to call attention to matters of this kind, but to my mind it seems either the business of science to do so, or at least the science of business. As manufacturers it has always been our business to seek to raise the standard of American manufacture, to seek to raise the standard of the cement. It has been our business to provide testing machines at our own expense wherever we could get engineers to use them; it has been our business to endeavor to get the railroads of the country to prescribe standards of cement, and we are ready to co-operate with any and everybody, consumers or engineers, if we can only get to some understanding and if we can come to some conclusion that is precise, accurate and definite.

Now, as to the quotation that Mr. Croes has made, that Captain Maclay does not propose any change in the mode of testing, but only writes his paper by way of a suggestion, I would only quote from the second sentence in Captain Maclay's paper, viz.:

"The object of this paper is to show that the test generally employed heretofore to determine the change of volume in Portland cement is of very little practical value, and should be changed to a steam and hot-water test." This certainly means something, or it does not. It has also been stated by Mr. Croes that all that I have mentioned in reference to Rosendale cements or natural cements has no reference whatever to Captain Maclay's paper, which is entitled, "Hot Tests for Determining Change of Volume in Portland Cement." Captain Maclay may have intended to confine his remarks exclusively to Portland cement, but the authority he quotes, and quotes approvingly, M. Le Chatelier, says, on page 423 of the paper presented to-night: "The use of the hot tests presents an absorbing interest to all manufacturers who wish to follow closely their own productions; above all, to the manufacturers of natural Portland and of hydraulic limes."

This certainly means something, or it does not.

Mr. Arthur Marichal.—I have read with interest the valuable paper by Captain W. W. Maclay on the subject of hot tests of cement, and was agreeably surprised to find his results to be almost identical with those of some experiments I had occasion to make in 1881–83, while investigating the possibility of shortening the time required for the setting. It being nothing else but a slow process of chemical combination, and such combinations being, as a rule, hastened by the presence of heat, the idea of hot-air and hot-water tests was suggested.

The briquettes were kept for several days in water at different temperatures, and, as a matter of fact, the addition of a very few degrees of heat caused a decided reduction of the length of time required for setting. But I was unable to discover any well-defined relation between time and temperature. Finally, the briquettes were kept in boiling water and the results became quite interesting. At the end of ten days the cement, when of good quality, had attained the strength corresponding to six months in cold water, and in the case of a very fine ground French Portland, that strength was already attained after seven days.

Chemical analysis disclosed the fact that the percentage of water entered in combination, after ten days in hot water, was the same as for six months in cold water, and that the strength of the cement was increasing with the amount of water entered in combination. It was discovered incidentally, that cement containing over 5 per cent. of magnesia, or 3 per cent. of uncombined lime, would not stand the boiling test.

Another peculiarity was disclosed while experimenting upon a cement containing a large percentage of ground slate: it took twenty-three days to reach the strength corresponding to six months in cold water. For cements to be exposed to air alone, I thought it advisable to experiment with hot air; unfortunately, the tests were not carried above 150 degrees, and the small number of specimens experimented upon (about one hundred and fifty), does not allow positive conclusions. But it was noticed that, in this case, the strength of the cement was increasing, not alone with the combined water, but also with the amount of carbonic acid entered in combination.

E. Sherman Gould, M. Am. Soc. C. E.—A method of testing the quality of a cement in twenty-four hours is the desideratum of the day for cement users. Although the testing machine for breaking briquettes by tension has, in connection with the sieve, done great service in raising the standard of manufacture, it is certain that it is not adequate to determine, satisfactorily and quickly, the value of cements. It may be doubted if its most useful service is not confined to comparing a cement with itself, that is to say, knowing that a certain cement should possess a certain tensile strength at the end of,

say, one week; if a given sample of such cement realizes this degree of strength, we naturally assume that the lot which it represents is in good condition, not deteriorated or shop worn, and up to its own general standard. In actual work, when it is sometimes difficult to obtain cement as fast as it is wanted, a method of testing which requires from one to four weeks to execute is simply out of the question. This more particularly as regards Portland; for natural cements can be made to give results—though often delusive ones—in twenty-four hours, with the testing machine. It is pretty safe to say that the tensile test has rejected many good, and passed many bad, cements.

Especially do the results obtained by the testing machine (when standing alone) fail as means of determining the character of a new brand of cement, and it is in this respect, particularly, that it is behind the requirements of the present day, when new brands are being continually offered in the market. It fails in principle, in rapidity, and as offering such a wide latitude to the "personal equation." But let us dwell no longer upon the defects of an old and esteemed friend, that has been a tower of strength in the past, and is destined to do good auxiliary service in the future.

It is a disappointment to me to learn that Captain Maclay, in the present paper, finds that there is but little assistance to be derived from chemical analysis in the important point of fixing the amount of free lime in Portland cement. It seems that the popular belief that one can find out everything about anything by "having it analyzed" is as delusive as regards cements as we know it to be as regards water.

It seems that the boiling test described in this paper bids fair to give us just what we want in the way of a quick test, and also a reliable one of at least one of the most important qualities of a cement. It certainly is severe enough, one would suppose, because boiling is far beyond any ordeal to which the cement can be subjected in use. Scientifically, this is all right; the test should be more severe than the use; the question would, however, introduce itself whether practically it was not so severe as to militate against its own usefulness. I think that a long and careful series of experiments should be made upon well-known and approved brands before any positive decision is come to respecting the merits of the system. We know that there are many good brands of Portland cements in the market, which are probably as good as, at present, cement can be made, and which years of experience have proved to be fit for any work executed by man. Let these be tried by the test, and note the results. We should then know whether the test would pass a good cement, which is as necessary as it is to know if it rejects a bad one. Partially, Captain Maclay provides for this, by stating that certain of the results may be ignored.

The above leads me to raise a point which, I think, is one of very interesting inquiry. Probably the amount of tested cement used all over the country is small in comparison with the untested. It is also certain that much cement is used, which would be rejected by a very elementary test. What statistics have we of work going to pieces or failing in one way or another, from the use of defective cement; cement, say, that would check, or that showed low tensile strength? I cannot but think that in many cases cements that would be rejected (and very properly, too) on account of failing to fulfil even the tests in common use, have still a chance of doing good service in the wall, or, at least, of not disrupting it by change of volume. I think this may be due—if it be a fact—to the circumstance that (speaking more particularly of Portland) the large quantities of mortar mixed at a time, secure an average quality; the great amount of manipulation it receives before going into the work; and to the fact that the larger part is imbedded in the heart of the wall, under shelter as it were, and setting under heavy pressure, all combine to give the actual mortar a much better chance than the briquettes. Probably many of the failures of masonry are attributable to the fact that there was not enough mortar used, rather than to the quality of the same. It must be a very good cement, indeed, that will hold a wall together when it is not there!

I note that the process as described seems to be limited to the detection of free lime. Does it also detect an excess of magnesia?

It is with great satisfaction that I observe in this paper a recognition of the fact, which I do not recall meeting elsewhere in my reading, that cements should be tested for air exposure alone as well as for behavior under water. Hydraulic cements set harder in water than out of it, and yet I believe that in few cases are tests made on unimmersed briquettes, even though so large a proportion of our masonry work is built above ground, and consequently removed from the beneficial contact of water. I feel sure that those who have never experimented with unimmersed sand-and-cement briquettes (and by this term I mean briquettes kept entirely away from water after gauging, and not covered with a wet cloth) would be surprised if they would leave a few "one to three" briquettes standing in the cement room for two or three months, with no wetting but what they got in the gauging, breaking them at usual intervals, and noting the results. Especially would they be admonished thereby to keep all air-exposed concrete thoroughly wet by sprinkling after being placed, for a period of time that I will not further designate than by saying, the longer the better, particularly in hot weather.

If the boiling process is to be the coming test, it will be in order to produce a handy practical apparatus for applying the same. It will probably be found that this test will not wholly supplant all others, but that, in carefully conducted experiments, the testing machine,

cold pat, chemical analysis, and boiling will all come into play. Indeed, I do not understand Captain Maclay as claiming for this test more than that it should be added to those already in use.

As it is to be hoped that this discussion will not be strictly limited by the title of the paper, but will elicit the views of members upon the whole question of cement testing, I will mention a test without which I think no examination of cement is complete. I mean that for second set of mortar. Thus, mix a lot of cement and sand in given volumes, say, ten fluid ounces of cement and thirty ounces of sand, wet and temper the mass as if for moulding, only instead of putting directly into the moulds, let it remain, if Portland, from three to six hours on the slab. By this time, unless it was mixed very wet, it will be quite dry. Add more water, if necessary, retemper and mould. Give the usual twenty-four hour air exposure, immerse, reserving a few briquettes for all-in-air test, and break as usual.

In testing a new cement, it is always well to parallel the tests with similar ones made with a standard brand, letting the treatment, step by step, be as nearly identical in the two cases as possible.

S. Bent Russell, M. Am. Soc. C. E.—It will generally be admitted, no doubt, that constancy of volume is a good thing in Portland cement. It seems fair to say also that any method of treatment which develops the tendencies to swell is a proper test of a cement, for the cement which swells the least under such treatment is entitled to the most credit. The best method for such purpose would be the one which departs the least from the treatment which the cement will receive in the work for which it is intended.

I have recently completed a set of experiments, the results of which will be interesting in connection with Mr. Maclay's paper. These results are shown in the table on the next page.

Brand No. 3 is a slag cement, brand No. 8 is an American Portland, and the others are German Portlands.

All mixing was done on the "jig," a mechanical mixer which has recently been described and illustrated in our *Transactions*. The term "pressed," as used in the table, indicates that the briquettes were put in the mould and compacted by a machine devised by the writer for the St. Louis Water Works Extension.

Briquettes A_1 and A_2 were made by pouring dry cement in the barrel of the press and adding the right amount of water. The mould was then set on top of the barrel and covered by a stop plate. By moving a lever a plunger was then pushed up from the bottom of the barrel, lifting the cement and thus filling the mould and compacting the material at one operation.

Briquettes B_1 and B_2 were made by dropping into the barrel of the "press," the ball of mixed cement as it comes from the jig. A mould of the American Society of Civil Engineers' pattern was then laid over the barrel and covered with the stop plate, and a stroke of the plunger

Tests of Portland Cement.

Brand.	Average Tensile Strength of 5 Briquettes, 7 Days Old.									Swelled.	Per cent. Fineness.	Average tensile strength of 10 briquettes	
	Pressed without Mixing.			Mixed and Pressed.			Mixed, but not Pressed.					Neat in 7 days.	1 to 3 sand in 12 months.
	A_1	A_2	Dif.	B_1	B_2	Dif.	C_1	C_2	Dif.				
1	2	3	4	5	6	7	8	9	10	11	12	13	14
1	202	306	104	634	566	...	581	583	2	A_1	86.5	606	285
2	137	327	190	634	775	141	559	579	20	A_1* and B_1	75.2	692	307
3	159	286	127	338	357	19	331	343	12	A_1†	94.1	376	341
4	137	332	195	446	468	22	423	459	36	A_1‡ and B_2	88.6	665	348
5	184	348	164	500	454	...	445	422	...	A_1	87.6	560	386
6	204	390	186	463	615	152	533	606	73	A_1 and B_1	89.5	593	426
7	346	518	172	519	689	170	571	578	7	A_1 and B_1§	91.7	675	452
8	234	365	131	317	525	208	310	562	252	A_1, B_1 and C_1	89.0	542	....¶

* 1 briquette crumbles; value in column 2 is average of 4 briquettes.

† 3 briquette crumbles; value in column 2 is average of 2 briquettes.

‡ 2 briquette crumbles; value in column 2 is average of 3 briquettes.

§ B_1 only slightly swelled.

¶ No 12-month test; strength in 3 months was 29.4.

Note.—A_1, B_1 and C_1 were immersed after mixing.

A_2, B_2 and C_2 were kept in air 24 hours before immersing.

Columns 4, 7 and 10 show the loss of strength due to quick immersion.

filled the mould. The stop plate was then removed and the mould lifted by a further movement of the plunger so that it could be taken off, inverted on an impervious table and struck with a palette knife. This completed the briquette. This is the regular method of making briquettes now used in our laboratory. The cement is quite plastic when put in the press, so that a small amount of water flows out of the joints as the greatest pressure is applied.

Briquettes C_1 and C_2 were put in the mould by hand, as in the usual practice.

Ten briquettes of each kind were made for each brand. Five of these were at once immersed in water, mould and all. The other five were kept in air for twenty-four hours and then immersed. All were broken when seven days old. Of the briquettes which were immersed before setting, it was noticed that a majority swelled within a few hours. All of them which had been made without mixing by the method first described were badly swelled and some had gone to pieces. Briquettes mixed and pressed as by the second-described method of five of the brands were swelled, while three of the brands showed no change of volume. Briquettes mixed and put in the mould by hand were not distorted by immersion before setting except in the case of No. 8, which was the American Portland.

Results almost parallel were found on breaking the briquettes. As shown in the table, the loss of strength due to immersion before setting was great with all brands when moulded without mixing. When mixed and pressed, some brands showed no loss of strength, while others were decidedly weakened. The American Portland was the only brand showing much loss of strength when moulded by hand. After making the tests shown in the table, a sample of No. 8 brand was air-slacked for one week to see if that would improve it. The results showed a decided loss of strength due to the exposure to the air, while the change of volume seemed as great as ever.

I should like to hear from Mr. Maclay if he has tried air-slacking with the brands used in his experiments, and with what effect.

That the first two tests are severe is shown by the fact that brands Nos. 1, 4 and 7 are of the highest reputation in this part of the country. Columns 12, 13 and 14 show some tests made of the same brands, but not from the same barrels. These tests were made with briquettes made over a year prior to the ones recorded in columns 2-11. They are added to give the reader an idea of the comparative value of the cements. It would seem evident from these tests that the swelling of a briquette depends partly on the character of the cement and partly upon the mixing and moulding.

W. W. Maclay, M. Am. Soc. C. E.—Mr. Lesley's discussion of the paper seems to the writer very objectionable on account of its many inaccurate statements and quotations respecting scientific and professional matters.

Mr. Lesley begins his paper by stating, in a general way, the desirability of having the cement manufacturer take part in the cement discussions with engineers. Where the manufacturer is a skilled scientific expert in his own line, and a man of liberal ideas and education, such co-operation as Mr. Lesley suggests might be permitted. Even then the financial interests of the manufacturer will often conflict with the impartial effort of the engineer to advance the quality of the product and to improve the method of testing in order to sharply separate the good from the bad.

Mr. Lesley says there is nothing stated nor explained in the paper in regard to results for longer periods than seven and twenty-eight days. No explanation was deemed necessary on this point because seven days for the hot tests and twenty-eight days for the cold tests were considered convenient periods and sufficiently long for comparing the two systems and for showing their marked divergence in case of bad cements. To make the hot or cold tests for any longer periods would practically prevent their use in general practice, without adding very much to our information on the subject of how soon mortars made from bad cements, will fail in the work. This knowledge cannot be expected absolutely from any system of testing, within reasonable periods, or, as one author tersely expressed it, "There is at present only one way of determining whether the judgment passed on a cement by any system of testing is sound, and that consists in waiting half a century to see how the work stands."

Sometimes this proof is forthcoming at an earlier period; for example, a few months ago the chief engineer of one of our most important public works, where they had been using large quantities of American Natural Portland cement, consulted the writer in regard to certain failures that had been observed in the briquettes made of this Natural Portland cement, both neat and gauged with sand, that had been kept in the laboratory for several months. This cement had passed the standard cold pat test, without showing any change of volume, and yet the briquettes after several months immersion in cold water began to swell and crack. In the work in which the cement was used, the writer was informed that no failure had been observed (possibly, however, this was due to imperfect examination) for as the sand briquettes cracked as well as the neat, one would have a right to expect failures, similar to those shown by the sand briquettes in the laboratory, to appear promptly in the work. That this work will ultimately show some failure, I think no one can reasonably doubt. The writer, therefore, advised its engineer to use the boiling test, which showed at once that the cement was unsound, and indicated, by the hot pats and briquettes cracking and swelling, the presence of free lime in large quantities, thus showing clearly in a few hours, defects of the cement that had become apparent only after several months observation of the cold tests, and completely

sustaining the theory advanced in this paper of the superiority of the hot tests to quickly separate good from bad cement. Here was a case where theory and practice agreed; theory told us, as Portland cement engineers and manufacturers know, the great difficulty in making Natural Portland cements which do not contain a large excess of free lime, and the great difficulty of making them uniform and reliable like Artificial Portland cements of the best quality. The practical boiling tests sustained this theory by showing in a few hours in the case of a poor Natural Portland, the failures that might be expected to take place in the mortar made from it, even when it took several months, or longer periods, for these failures to appear by the cold test or in the work. In tests by the cold method of large quantities of cement used at Havre, extending over eighteen years (1871 to 1889), the cements which turned out least well after long periods, satisfied the cold test, during delivery, quite as well as those which endured the best.

Having alluded to the difficulties of proving, by the results in the work, that the cements rejected by the writer's system of hot tests are generally bad, and that cements passing these tests are generally good, he has endeavored to obtain this proof in another way as stated in the paper, that is, by analogy; and it has been proved repeatedly by the writer to his entire satisfaction by experimenting on all the brands of Portland cement in this market, that the old-established brands of deservedly high reputation almost invariably pass this test, and that new brands of little reputation, and cheap on account of their being untried, are the cements that generally fail. Cements coming from factories located where the raw materials are of poor quality and not homogeneous, are, as would be expected, almost invariably rejected, for no amount of care in the process of manufacture can compensate for poor raw materials.

Mr. Lesley in quoting from page 129 of E. Candlot's book, quite alters M. Candlot's words, at the end of his quotation. Mr. Lesley finishes this quotation as follows: "The use of Portland cement, which is much larger in Germany than in France, has never given rise to disasters." M. Candlot's real words are: " * * * the use of Portland cement, which is made upon a much larger scale in Germany than in France, has never given rise to disappointment. The only disasters that have been reported, have been produced by magnesian cements." Again, Mr. Lesley says in a paragraph intended to be humorous: "While M. Candlot seems to be willing to agree with any and everybody as to temperatures, but expresses, as does M. Le Chatelier (*Bulletin de la Société D'Encouragement*, etc., August, 1890) very great doubt as to the utilization of this test at all among engineers and consumers, but relegates it with M. Le Chatelier to the manufacturer as something very interesting for him to amuse himself with," etc. Now, nothing can be farther from the sense of the writings of MM. Candlot and Le Chatelier, than the views attributed to them by Mr.

Lesley. In the closing pages of the writer's paper can be found the opinions of M. Le Chatelier on the hot test, and the following translation of the part of page 149 referring to the subject in M. Candlot's book certainly does not justify the language used by Mr. Lesley in this connection. M. Candlot says:

"After all, it seems to us that the hot tests would give useful information, on condition of not adhering solely to the test of pure cement, immersed in boiling water. If the cement resists well this test, its constancy of volume is certain, but if it presents traces of swelling we should ascertain by tests of mortar 1 to 3 immersed in water, at 80 degrees (Centigrade), if the cement really contains uncombined lime or if it is only a matter of a cement, imperfectly burnt, but presenting a proper composition and homogeneousness." This is exactly what the writer's paper recommends, and it seems strange that such plain language can be changed into the meaning given to it by Mr. Lesley.

Mr. Lesley is entirely mistaken in his remarks, that no one, in connection with the boiling test, has considered the effect of fine grinding, and the amount of water that should be mixed with the cement, in order to make the pats. If he will look at Table No. 1 in the paper he will find the fineness given for each of the twenty-three boiling tests, and he will also find that the finest ground cements do not fail on the boiling tests any more than the coarse. He will also find in the paper that it is stated that both the pats and briquettes for boiling are made of the same consistency, as the briquettes for tensile strength by the cold method, that is, all cements have sufficient water added to them, to make a stiff plastic mortar or paste. The object in all tests referred to in the paper was to gauge the cements, both pure and with sand, for the boiling test, exactly the same as for the cold test, and as the latter was never made with too little water, neither did the boiling test ever suffer, in that respect. Mr. Lesley's quotation from the paper read by Rudolph Dyckerhoff, is unfortunate, for, in the paper referred to, Mr. Dyckerhoff in speaking of the tendency of the pats of finely ground cement to shrink, alluded to their behavior upon exposing them in the open air after their removal from the water in which they had first set. This condition, exposure in the air, is of course favorable to shrinkage, both for coarse and fine cements, and has not a single parallel with the boiling test, where the cement is first gauged with a full amount of water, is then allowed to set in a moist atmosphere, then kept three hours in steam, then immersed in nearly boiling water until the end of the experiment, and never for a moment, purposely, are the pats or briquettes allowed to become the least dry. This is a most important point and will be alluded to in another connection. Mr. Lesley gives a long quotation from the proceedings of the German Manufacturers' Association, held in 1891, where it was unanimously resolved "to adhere to the normal test for Portland cement." The

weight of this unanimous action of the manufacturers in causing engineers to abandon the boiling test may be somewhat lessened when we learn that at the same meeting one of their members stated that "in Russia, a short time since, a specially appointed commission, on the adoption of Portland cement for harbor works, recognized 3 per cent. of magnesia as permissible. But, then, several Russian factories made up to this time a Portland cement with over 3 per cent. of magnesia, so for another year 5 per cent. is permitted (by the Russian standard) to allow these factories time for a gradual transition to cement with 3 per cent. magnesia." Might not the same reasons have induced the German manufacturers to vote against changing the normal test, because some of the factories could not make cement to pass more rigid requirements. Another point in regard to this resolution requires some explanation. It is that, while adhering to the normal test, they say that the accelerated tests may be useful to the manufacturer. Now, this is really a step in advance of the boiling test, because the accelerated tests referred to in the "Proceedings" consisted of several distinct hot tests; namely, the red-heat test, the kiln test, the high-pressure steam test, and the boiling test. Of these, the boiling test, as described by the writer, is very mild as compared with the red-heat test and the kiln test, both of which were used in the experiments quoted by Mr. Lesley from the proceedings of the German manufacturers in this connection, and, therefore, the said experiments have no relation whatever to the paper of the writer. For how could any one tell, after exposing the samples to the dry red-heat test or the dry kiln test, what effect the boiling test would have on them? If there is any one fact known about the setting of Portland cement, it is the necessity of giving it the full amount of water required for the chemical action to take place in its first hardening. To deprive the cement of water at this period is, of course, fatal to its cohesion, and yet this is precisely what is done in the red-heat test and the kiln test, both of which constituted the so-called acceleration tests which are quoted with so much emphasis by Mr. Lesley. In this quotation he has substituted the word "hot" instead of "kiln" in the following lines: "It has occurred that cakes of a cement which had not endured the hot (this should be kiln) test, have been preserved safe and sound up to date, over three years." This substitution, inasmuch as the writer's paper is entitled "Hot Tests for Portland Cement," conveys the idea that the same tests are referred to; whereas, they have nothing in common. In the closing part of this discussion of the accelerated tests by the German manufacturers, the following remarks were made by Dr. Erdmenger, which Mr. Lesley avoided quoting, and which exactly express the writer's views on the subject; the high-pressure steam test, as practiced by Dr. Erdmenger, closely resembles part of the boiling test described in the paper, except that the high-pressure steam test is much

more severe, any cement passing the high-pressure steam test, therefore, will surely pass the boiling test. Dr. Erdmenger says, "I wish to dispute Dr. Schuman, who pretends that somewhere I have said that a really perfect cement also might become defective by boiling. This is not the case. I know most of the brands represented here. I shall not mention in the meeting the names of the brands that have stood the high-pressure steam test; but I could name a whole list of the brands represented here, which have stood the high-pressure steam test in the sand test up to forty atmospheres, and several brands which have stood this high pressure in the neat test. Therefore, perfectly faultless cements do not show any weakness from the boiling tests. Moreover, some cements that stand the sand tests show some derangement when tested neat, although the swelling tendency is not sufficiently pronounced to say that they are bad in ordinary practice. However, these cements are of the second rank. The genuinely excellent first class brands will not show any defect. Therefore, perfect and excellent cements will not be boiled to pieces by the high-pressure steam test. Cements which in the sand tests cannot endure a boiling of about twelve atmospheres, ought to be rigorously designated as of faulty make."

Mr. Lesley is again unfortunate in the report which he quotes of Professor De Smedt, as it is fairly bristling with errors for so short a paper. Mr. De Smedt says that "Several well-known methods of determining free caustic lime (CaO) in hydraulic cement, are already at our disposal, whereby the safety or unsafety of a cement can be pronounced without any difficulty," etc. Besides, the authority of Fresenius (quoted in paper), who says, "As yet no method is known by which the object here stated can be accomplished with absolute accuracy," the following letters are submitted as confirming the writer's view of the question. These letters were written last year to a large manufacturer of Portland cement in Europe, and by him were sent to the writer.

From E. Candlot: "I am pleased to reply to your letter of the 9th instant. There is in fact no method for finding the percentage of free lime in the cement (referring to Portland)." * * * * From Dr. Schuman: "In reply to your favor of the 6th instant, I beg to inform you that I do not know a method for finding out the percentage of free lime in Portland cement. I do not think there exists such a method, and I am myself of the opinion that chemists will never find out one; the solutions capable of taking away the free lime from the cement will always work in a more or less strong degree on the cement itself."

Under these circumstances, if Professor De Smedt really has a reliable method for determining the free lime, he should publish it for the benefit of experimenters who are looking for one. Professor De Smedt was unfortunate in getting natural cements to pass the boiling tests. Many natural cements contain a great deal of uncombined lime, and,

therefore, will not pass the boiling tests; but the writer's experience is that the better grade of Rosendale cements invariably pass this test.

Mr. De Smedt infers that his twelve samples of Portland cement should be adjudged bad by the boiling test, because the pats left the glass. It is distinctly stated in the paper that this requirement (adhering to the glass) is not insisted upon although it is generally found with perfectly faultless cements. It was not the intention of the writer to discuss in his paper the different theories of the setting of Portland cement, which is a strictly chemical question, about which opinions widely differ and which has really very little to do with the boiling test. Prof. De Smedt's assertion that "pure aluminate of lime, heated to 200 degrees Fahr., bursts and dissolves into a powder, while setting," if it is proved, shows that the aluminate of lime must be present in some other condition, in a perfect Portland cement, which can be heated to 200 degrees Fahr. without bursting or changing its form anyway. The best French cement chemists, particularly Bonami, consider the aluminate of lime when it is present in a very basic condition as a direct source of expansion, exactly as much as free lime, and that in a perfect Portland cement we should have the double salt, silicate of alumina and lime, which is more stable than the aluminates.

In relation to Mr. Lesley's suggestion that as slag cements pass the boiling test, and his own experience shows failures with the slag cements, therefore the boiling test is unreliable, the writer can only say that his experience with slag and pozzuolanic cements extends over a period of eight years, and that he has never had any slag briquettes swell or disintegrate; on the contrary, they have been exactly as stable as good Portland cement, and the sand mixtures have been much stronger. The following experience of Mr. M. L. Holman, in relation to the behavior of slag comments, confirms that of the writer.

Mr. Holman says: "In my experience with slag cements I have never run across any that swelled, either wet or dry. We have some work in this city (St. Louis) made of slag cement, which is now about five years old and thus far no sign of swelling has appeared. This work is in sidewalks exposed to the weather. I have on hand briquettes about three years old that have been kept in air and they are yet sound. These are both neat and with sand (3 to 1). We also have a considerable amount of concrete work, brick and stone masonry with slag cement, both wet and dry, some now four years old that show no sign of failure." Mr. Lesley's argument that the failure of cements by the boiling tests may be due to the aluminate of lime acting like quicklime is not the theory entertained by the best cement chemists, although, as the writer previously stated, aluminate of lime in a very basic condition will act like quicklime; but then it should not be present in this condition in properly made cements, and the boiling test is therefore very useful in pointing out this fact.

Mr. Lesley's treatment of the function that aluminate of lime plays in the setting of Portland cement is greatly exaggerated, but not so much as his vision of the future field to be occupied by aluminate of lime cements where their growth would be much retarded by the introduction of the boiling test. He says no Portland cement of this kind (aluminate of lime cements) are made in this country—a very safe remark, for no Portland cements of that kind are made in any country, nor will they ever be made, because of the very nature of Portland and other high-class hydraulic cements capable of resisting the action of sea water, in which the essential constituents are lime and silica, and the incidentals, alumina, iron, etc. In the Proceedings of the Institution of Civil Engineers, Vol. CVII, there appears a most interesting paper by Dr. W. Michaëlis, on "The Behavior of Portland Cement in Sea Water," in which it is very clearly stated that cements and hydraulic limes, rich in silica and as poor as possible in alumina and ferric oxide, are to be preferred for sea construction; because "the aluminate and ferrate of lime are not only decomposed and softened rapidly by sea water, but they also give rise to the formation of double compounds which, in their turn, destroy the cohesion of the mass by producing cracks, fissures and bulges." Cements of this kind crack or swell at once in the boiling test, thereby giving immediate notice of failures which will take place ultimately in any sea work, where the cement may be used. Opposed to Mr. Lesley's diatribe against slag cements, and his preference for the mythical aluminate of lime cements, is the well-known fact mentioned by the writer in some of his other papers, and also by Michaëlis; that, while in the Mediterranean, works built of hydraulic limes and pozzuolana rich in silica, have remained unaffected by the salt water, the hydraulic lime works for over one hundred years and the pozzuolana for about two thousand years, nearly all of the Portland cement constructions in that sea have been destroyed within a comparatively short period, because the Portland cement contained an amount of soluble aluminate of lime, large in comparison with that in the hydraulic limes and pozzuolana. Now, the slag cements are not only identical with the pozzuolanic in chemical composition, but the method of combining the slacked lime with the silica is the same in both, only the silica compounds in the slag are artificially made and in the pozzuolanic they are natural. The slag cements therefore, while they more closely resemble in chemical and physical composition the the old Roman cements of the Augustan era than do the Portland cements, are very cheaply made and will always be formidable rivals of the latter and be roundly abused by the Portland cement manufacturers.

Mr. Lesley mentions the French cement of Vassy as an illustration of the aluminate of lime type referred to. Now, the cement from the Vassy district is what we call a quick-setting natural cement, contain-

ing about 23 per cent. of silica, 52 per cent. of lime, and about 9 per cent. of alumina, and considering this chemical composition it is difficult to understand how it can be classed as an aluminate of lime cement. Mr. Lesley refers to Mr. Faija as having originated the steam and boiling test described in the paper; now, no one is more willing than the writer to give Mr. Faija his full share of credit for all the good work he has done for Portland cement, but in the tests for determining change of volume he has simply been a worker in the same field with many others, among whom may be mentioned Michaëlis, Tetmajer, Chatelier, Deval, Candlot, etc.; and, as a matter of fact, the writer used the steaming process to determine free lime before reading Mr. Faija's first paper describing his own process; and the experiments described in the writer's present paper, as well as the paper itself, originated from reading the report of M. Chatelier on the experiments of M. Deval. Mr. Leslie calls Mr. Faija the pioneer to adopt the boiling test, and at the same time refers to his, saying, "He (Faija) had lately made hot water tests of cement, and found that he could blow any cement to pieces if it was subjected to the specified temperature of 180 degrees long enough, and, therefore, boiling was not indicative of bad or good cement." This is a general statement and rather a strong one, coming from the pioneer to adopt the boiling test, in refutation of which the writer can only say that he has in his laboratory many Portland cements that neither Mr. Faija nor any one else can boil to pieces in seven days' boiling. What might be the effect of longer boiling than seven days, several months or a year, for example, he does not know. Mr. Leslie says that the only supporters of the writer's paper are two French writers, and not a single authority in Germany, England, Belgium and the United States. In the two latter countries there are very few authorities on Portland cement. In England Mr. W. G. Margetts, of the West Kent Portland Cement Company, states that he has used the hot water tests for over a year with very satisfactory results in improving the output of their works, and says: "It had hitherto been deemed a sufficient safeguard in addition to the usual cold water tests, to analyse the cement with a view of ascertaining the quantity of lime in its composition, but his firm had found from long experience that this idea was fallacious." In Germany, besides Dr. Erdmenger, already quoted, the advocates of hot tests are Dr. Tetmajer, of Zurich; Dr. Schott, etc., and, last but not least, Dr. Michaëlis, whose own paper, December 15th, 1891, entitled, "Is the ordinary pat on glass test sufficient evidence of the blowing of Portland cement?" contains the following: "It is further proved thereby that there are better tests for constancy of volume than this 'pat on glass' or 'regulation test,' and that Professor Tetmajer was entirely right when, in his excellent report to the Sub-Committee No. 12 of the Second Standing Committee for the Uni-

fication of the Standard Methods for testing Building Materials, he described the boiling test for Portland cement as the only test which is absolutely trustworthy. I had myself proved this, after many years' experience, and had, therefore, given prominence to this test, and recommended it, and I have employed it myself ever since" etc. This report of Professor Tetmajer, so unqualifiedly endorsed by Professor Michaëlis, the leading authority in the cement world, is the one that Mr. Lesley assails in his discussion as the freak of an enthusiast.

The writer intended to confine this paper to the effects of the boiling test upon Portland cement, but from numerous experiments he is convinced it can also be applied to natural cements with much satisfaction. Mr. Marichal's remarks are very interesting, and the fact of his finding the boiling test would reject cements containing over 5 per cent. of magnesia is in accord with the claims of the German advocates of this test, although the writer has not made many experiments in this line himself. The question of Mr. E. S. Gould, therefore, "Does hot test detect magnesia?" the writer thinks can be answered in the affirmative. In relation to Mr. Gould's recommendation to test cement mortars by allowing them to take a second set, the writer cannot approve of it on account of needlessly complicating the subject of testing as at present practiced. Mr. Russell's experiments are entitled to much interest, but the writer does not think that the results to determine change of volume are obtained in as simple a manner or as accurately as by the hot tests. The writer has tried air-slacking the cements exposed to the boiling test, and invariably with better results than without air-slacking; but he does not consider air-slacking a sample in the laboratory fair practice, unless it is intended to air-slack the cement going into the work for a similar period.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

556.
(Vol. XXVII.—October, 1892.)

A METHOD OF TUNNEL ALIGNMENT.

By H. F. Dunham, M. Am. Soc. C. E.

Read May 4, 1892.

WITH DISCUSSION.

While engineer in charge of tunnel work, difficulties in alignment were diminished by certain methods that may be of interest, if novel to others, as they were to the writer.

The alignment of a timbered tunnel is established and perfected by the alignment of its wall plates. The method at first employed was to extend a center line as the work progressed, giving points upon the floor of the tunnel or the roof of the heading, and from these, by the use of plumb lines, measurements were made to fix the lateral position of the wall plates. The grade or elevation of the plates was afterward fixed by a common wye level or by a transit provided with a level.

A tunnel even in firm material is a difficult place in which to preserve bench marks and reference points. The floor of the tunnel for a considerable distance back of the bench is subject to more or less disturbance from attention given to track work and the sorting and stor-

age of material. The floor of the heading is being constantly removed as well as encumbered with heavy material as the work goes forward. The entire roof, including wall plates, is subject to lateral as well as downward movements caused by removal of the bench and by unequal loading, against which no foresight can fully guard, and finally with every eight or ten hour shift an entirely new force of men, foremen and inspectors take up the work, usually with little regard for suggestions and directions left by the former shift. Under such conditions it is not strange that the alignment has often to be repeated and the elevation taken up frequently from some datum mark on the floor of the tunnel and transferred by long-rod or steel-tape measurements to the level of the heading, and thence to the wall plates. This involves much troublesome work, at all times in close quarters where the dust and smoke are unpleasant in their effects upon both men and instruments, and sometimes requiring a complete interruption of all work in a heading just when all other conditions demand the immediate prosecution of that work.

With a view to some improvement and saving of time, a strong platform supported upon 8 x 12-inch timbers set about 3 feet in the rock was erected at one side of the open cut and about 50 feet from the portal of the tunnel. The height and position of the platform were such that a common wye level when placed upon it would be a little above and inside the line of the wall plate upon that side of the tunnel. No attempt was made to fix the instrument in any exact position, but small holes were made in the timbers and protected from wear by washers for the tripod points, and one point was marked so that the instrument could be placed at any time in the same position it had before occupied. The distances from the center line of the tunnel and from the grade plane to the center of the instrument on the platform were then noted, and used in fixing the position of a permanent target back of the portal across a valley and about 1 000 feet away.

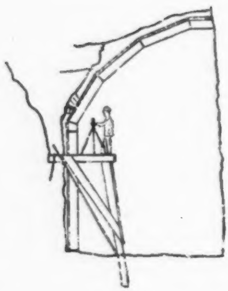
The distances already mentioned determined the relation of the instrument to the line of wall plate, and a gauge board was made of convenient form to rest upon the corner of the wall plate and to support a plummet lamp. A larger board of convenient form to span one-half the distance between the wall plates was also provided. The parts of both boards liable to wear were tipped with iron.

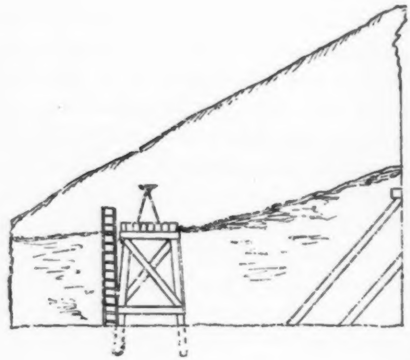
The work of alignment was thereafter as follows:

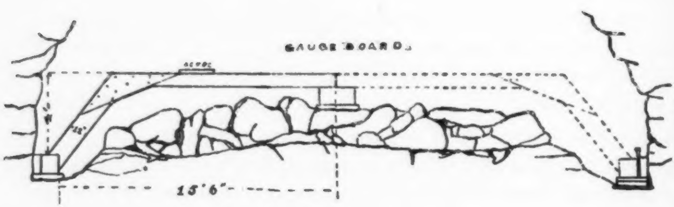
The instrument man placed the level in position on the platform, set and clamped it upon the permanent target, reversed the level in its wyes, and lined in the plummet lamp in the gauge board held upon the wall plate by the rodman, who kept the gauge in place at the end of the wall plate and directed the workmen to first raise or lower the wall plate; and after that it was moved laterally into position, *i. e.*, the bracket and wall plate were moved together as one piece, under the direction of the rodman and according to the signals of the man at the instrument outside the tunnel. After the wall plate upon one side had been secured in position, the larger gauge board and a spirit level were used to determine the position of the plate on the opposite side. Directions were given to have the required space, about 2 feet in width and 2 in height, on the floor of the heading and alongside the wall plate, kept free of timber and broken stone at all times. The remaining floor space was surrendered without restrictions, and there was little or no further interruption of the work of excavation by the work of the engineers.

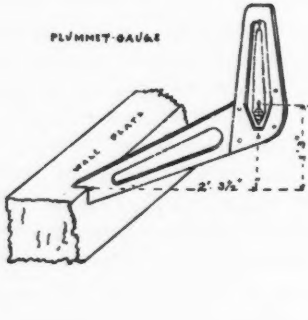
The saving in the time of doing the instrument work increased as the tunnel was extended, but a fair estimate would be at least three-fourths; that is, the work was done in one-fourth of the time required by the old method. At quite long intervels the center line was run in with a transit as a check upon the work accomplished with the level. The small plummet lamp could be seen, through any atmosphere suitable for men to work in, at a distance of more than 1 000 feet.

No trouble was experienced in the adjustment of the level used, but the telescope was reversed upon the permanent target after sighting the lamp. This method was used only in timbered tunnels, but it would be of service where masonry was required or where the material required no support. Good results may be anticipated from its use in any work from a portal. It could not be so easily adapted in tunneling from a shaft. With line and grade established at one point in a heading, there would be no difficulty in transferring both quite accurately to any desired point by the use of one or two light forms provided with spirit levels. It could also be done quickly with a straight-edge and a level. The advantage to a contractor of being able to ascertain at any time the lines of his work and to avoid removing material outside these lines is evident; and while the method here described is a step in the right direction, there seems to be an opportunity for further improvement.



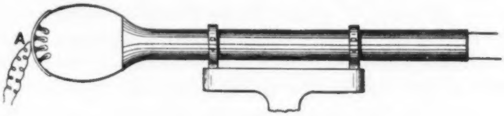



GAUGE BOARD.
15' 6"


PLUMMET GAUGE
WALL PLATE
2' 3½"

An instrument was used through which a lamp sent a feeble ray of light from the extreme end of the heading. An instrument was wanted through which a lamp should send a small but powerful beam of light into the extreme end of the heading. The instrument should converge and direct the light of the lamp. The beam might contain rays not absolutely parallel. At a distance of two or three hundred feet a disc with a small opening could be set up to intercept divergent rays. This and the instrument itself would become permanent features during the construction of the tunnel.

Untried schemes and conjectures are out of place here. It is the writer's purpose only to express a belief that such an instrument would be desirable, and that the use of electricity for lighting would be an important factor in its construction and operation.



DISCUSSION.

Robert B. Stanton, M. Am. Soc. C. E.—Mr. Dunham's paper is certainly interesting, as showing one of the many contrivances by which engineers get around difficulties not laid down in the text books.

In his enumeration of the difficulties of keeping the alignment in a tunnel while setting the timber, on account of the insecurity of the material, there is one that has come under my observation which seems to be omitted. It occurred in the construction of the Cincinnati Southern Railway, where the tunnels were being driven through the coal slate of the Cumberland Mountains. The roof was hardly ever secure enough to hold a permanent center plug. Besides the roof falling in, we had the further difficulty that the bottom would not stay down. The material through which the tunnels were being driven consisted of layers of coal slate varying from 3 to 10 inches in thickness. After the full section of the tunnel was taken out, the layers of slate in the bottom, first one and then another, would bulge up and break into pieces with a loud report and with such force that men sitting on kegs and boxes, drilling near the breast, were thrown over on the floor, and one man was toppled over as he was wheeling out a barrow of rock.

This seemed to be caused by the position of the strata of slate in the mountain on each side of the gap through which the tunnel was driven. Both sides sloped down toward the center and when the tunnel section was taken out, the pressure on the sides caused the bulging of the floor, and pushed the sills and posts out of line.

In such cases it would be difficult to keep Mr. Dunham's platform in true position, but I have no doubt he would find some other way of meeting the difficulty.

The careful alignment of tunnel work becomes a matter of vital importance, where it is necessary to set the timbering close up to the heading as the work proceeds. There can hardly be any rules laid down. The engineer who is competent to handle such work has, generally, ingenuity enough to get over his difficulties by wooing, or being wooed by, that old dame who is reputed to be the mother of invention.

For accommodation and convenience sake this alignment is generally done on Sunday. I never was, however, a believer in that new version of the fourth commandment, prepared especially for engineers on heavy mountain construction, which is, "Six days shalt thou labor and do all thy work, and on the seventh thou shalt do more work than on all the other six together." I am therefore thankful to say I never went a Sabbath day's journey or did a Sabbath day's work to satisfy the whim of any contractor. I have adopted the plan of doing such instrumental work early Monday morning, and requiring that no blasts shall be fired until it is completed, believing that the engineer is as much entitled to his rest as the negroes and mules on the grade.

O. F. Nichols, M. Am. Soc. C. E.—This paper is interesting as showing a method of alignment in tunnels, particularly where the curvature is not great. Of course, the greatest difficulties of alignment occur in tunnels with the greatest curvature, where the instrumental work is necessarily carried from point to point in the tunnel, and much interfered with by the work of construction.

I have no doubt that all engineers experienced in subterranean work have met the difficulties referred to in the paper in securing proper light, and I believe an instrument with satisfactory lighting has yet to be devised.

Mr. Dunham's suggestion, looking towards electrical lighting in tunnel instruments, indicates that he has the same feeling as myself, that we should look to electricity to solve this problem. The old-fashioned lamps are obsolete, or should be, on account of inefficiency, inconvenience and uncleanliness. Mr. Dunham's "wye-level" method for the special case where the instrument can be set up out of the line of work, and in the prolongation of the tangent, seems to have been ingeniously devised and executed.

Tunnel alignment develops, to a great extent, the ingenuity of the engineer, and this paper illustrates an application of this ingenuity.

All engineers will recall many experiences of absurd errors and defects in tunnel alignment. I recall one or two instances in Peruvian practice which may be of slight interest. In one instance a set of tunnels was under construction on a development line, the tunnels being so placed that two of them on the return line occurred immediately opposite, and considerably above, a lower tunnel on the advance line. The tunnels were so close together, both as to plan and elevation, that neither the upper nor lower could be constructed without seriously interfering with the others. All the tunnels were on curves; the engineering force was crippled from sickness, etc., and the tunnel work was protracted over a long period, the alignment and construction did not receive much attention from the engineers, often being entirely in the hands of foremen, who were frequently changed. The heading finally met, no one knew how. A species of burrowing was resorted to; a small drift being driven as crooked as the proverbial ram's horn, the wonder being the men did not lose their way in the mountains. When the tunnels were completed, much of this burrowing work was entirely outside of the finished section. In another instance an engineer ignored the original plan for alignment, and produced the tangent in a tunnel already begun far beyond the point of curvature intended, so that a great niche exists in the tunnel to-day, making it at this point twice the width of the normal section.

We lay too much stress, I think, on the strict accuracy of tunnel alignment during construction. Considerable latitude is allowable, and I have found that instrumental work at long intervals, say, embracing 30 to 100 feet of progress, will enable skillful workmen to construct the tunnel with sufficient accuracy. In curved tunnels I have often established points on the line, and given the foreman tables of tangent and chord deflections, from which practice I have secured very good results. Great accuracy is necessary in locating difficult tunnels and for purposes of professional pride in determining how nearly the headings finally meet.

This and all such matters, however, may be left with the experienced engineer. New, ingenious and often instructive details will, as in the paper before us, come out with each different problem, commanding the application of intelligence and skill in the execution of the work and eliciting great interest in their presentation to the profession.

J. F. O'Rourke, M. Am. Soc. C. E.—Mr. Dunham refers especially to a tunnel driven from an open portal. Perhaps it might be of some interest to tell you how the lines and grades for the south half of the Haverstraw tunnel, which was driven from a shaft, were given. We found there that after setting the plummet wires in position and then moving the transit down below, the wires were apt to move, so that when set in line with them it would be "off." Then, two

instruments were used. We had the line exactly located on the top; an instrument was set up close to the shaft, and the duty of the man there was to keep a constant eye on both wires. He found that perhaps every five minutes there would be some change requiring readjustment, small as regards actual measure, but very perceptible when it is remembered that the wires were but 12 feet apart and the line through them was produced 800 feet. The second instrument was taken down in the tunnel close to the shaft and shifted about until exactly in line with the two wires. Then, a point was cut in the rock at the back of the shaft and so arranged that a light could be placed on it, which was used as the back-sight. The fore-sight was put as far forward in the tunnel as conveniently possible, and cut in the roof; under this the instrument could be set and the line produced still farther from the back-sight at the shaft as the tunnel progressed. A number of repetitions of this operation just before the headings were joined gave scarcely any variation, and the lines finally met with an error of but $\frac{6}{100}$ths. But the way that we gave points to the men is the principal feature to which I desire to call attention. In addition to the centers that were marked for our own use, which were cut in as crosses, and their stations carefully determined, there were also holes drilled into the roof, in which were driven plugs, put in from time to time as the heading progressed, and their relation to grade was obtained with a level, and the distances from them to a plane a few feet below the roof grade at the center, was given to the foreman.

From these plugs he would suspend lanterns at the given distances below the plugs, and thus at any time he could see how the tunnel was going both for line and grade, and so also could any one else whose business it was to see that the work was being properly prosecuted, and in consequence at no time was the normal section exceeded by mistake. The north end, which was driven from an open portal, had 500 feet or more of 5-degree curve, terminating where the headings met, in 300 feet of tangent; the line at that end was produced from fore-sights perhaps 8 or 10 miles away. It was fixed by an iron plug in the bottom at the exact station of the portal, and was carried along from that point into the heading. All the tunnel line was located on a high, steep mountain, and it is questionable if the rough methods which some think are all that is necessary would have enabled the headings to meet.

Foster Crowell, M. Am. Soc. C. E.—I do not think I have any reminiscences such as Mr. Nichols and Mr. O'Rourke have given. The advantage that Mr. Dunham found in keeping his instrument off the ground is a very decided one, and I think, in all cases where the line of sight can be kept out of reach of the work that is going on in the body of the tunnel, it is extremely desirable. I think it makes very little difference about the exact alignment of the heading, provided the benches do not follow too closely on the heading; and if the

formation is such, that the heading can always be a considerable distance in advance of the finished work, the alignment of the heading, as long as it is within reasonable correctness, need not be very exact.

The question of throwing a beam of light along the tunnel as it advances, which would solve a great many difficulties, I think has already been solved in the electric search light which can readily be adapted, when the necessity justifies the expense.

There is another method which, I think, has not yet been tried. I think in a great deal of difficult tunnel work, where the difficulty is one of alignment, and where the depth of the tunnel below the surface is not too great to preclude the application of this idea, it is very feasible and would often be very desirable, especially in cities. Points can be transferred from the surface by drilling, using the diamond drill through rock, in advance of the excavation, and marked by metal rods, which can be reached and referred to, from time to time, as the work progresses. It is quite possible to drill with the diamond drill a very straight bore. It is not necessary that it should be exactly vertical, as long as it is straight.

After the alignment has been decided on, fixed on the surface, it is quite feasible to sink the holes with the diamond drill, and then, by means of a taut wire through the hole, the position of the lower end of the taut wire can be determined with great accuracy. Both line and grade can thus be transferred. It does not matter whether it is on the center line or not, the point can be very easily got at. When prospecting holes are drilled on the center line, they can be utilized. These are devices to be adopted in the tunnel era we are coming to, of tunnels under cities. Tunnels and the difficulties of following the alignment under cities, are usually much greater than the difficulties met with ordinarily in tunnels situate delsewhere; so, I think the engineer can look forward to a number of devices, in order to provide properly for alignment under whatever emergencies may occur.

A. McC. Parker, M. Am. Soc. C. E.—I have nothing to say on alignment of tunnels because I have never had any tunnel alignment to do, but I have done considerable underground surveying. One of the worst puzzles I ever met was the building of a timber track on a slope on a bad piece of foot wall, which ran at a considerable angle with the bed of our track. I had my center line carried down on a wire exactly on grade, but I did not know how to get the bed of our track right all the way up. I had a very ingenious mine carpenter working with me, and he suggested that we throw a point down about 150 feet with the transit, then go down and put a line in at right angles to the direction of the shaft and string two additional wires from this line, and take them out of wind with the wire we already had strung, and build our timbering to them. We took the measurement carefully at the side wire each time, and cut our timbers to fit, put the caps on, and they went on beautifully.

One of the gentlemen spoke of getting the line of tunnels by strings stretched from plugs, on curves. I once fixed up one of these devices for lines in drifting, and was congratulating myself on how easy it was going to be for the miners to carry it along. I said: "The next time you get ahead about 5 feet you want to be about 8 or 10 inches off that last line and you will be exactly right." When they fired the next round of holes in the face of the drift, they fired the whole business, and there wasn't a sign of a plug or anything else left to show that any such scheme had been tried.

In Colorado a shaft came under my notice which had been sunk by a lot of Kansas farmers. They had gone out and taken up what they thought was a valuable claim. They had found a ledge between two walls of granite, which cropped out on the surface, and they undertook to sink a shaft on this, at an angle of about 60 degrees. In some peculiar way they sunk and blasted, so that when they got down about 120 feet below the surface, the long dimension of the shaft was exactly at right angles to the position in which it started. Consequently, they thought that if they started off at right angles to the long dimension of their shaft they must certainly come to what they were looking for. After running in an easterly direction and not striking the vein, they ran in the other direction, thinking if they did not catch it on one side they would on the other. They ran at right angles with the long dimension of the shaft about 100 feet and then they went about as far in the other direction. It was all through hard rock too. By that time they were pretty well puzzled and their money pretty well exhausted; they came to me and asked me to go out and see what I could do to straighten them out. I went out, took my transit along, put a line down the shaft, and had just room to get one down and get a back-sight, and to my surprise I found the drifts running at right angles to the direction they should have run. I came back to town and told them it would take quite a while to calculate just what they wanted to do, and waited about two days, and then sent them a bill for my money. Then I said: "I will have to go out with you and show you just what to do. It was a pretty nervy thing to do, for I was afraid they might kill me when I told them. I had left a short line down in the two drifts and I took a piece of cord down with me and with a carpenter's square laid out a right angle from the line, and said: "Go in there about 2 feet and you will strike what you are looking for." They found it. This is only a sample of what people will do when they do not understand what they are trying to do. These men had drifted at right angles from their proper direction from the point from which they started at the bottom of their shaft, and they had wasted about $2 000; and about 2 feet away from any point in this 200 feet of drift they could have struck what they were looking for, if they had understood their business.

C. L. Crandall, M. Am. Soc. C. E.—The success of the method proposed by Mr. Dunham will depend to quite an extent upon the penetrating power of the target lamp used. Experiments on shipboard have shown that the electric light is the most penetrating when looking through fog. It should, therefore, show the best through the smoky air of a tunnel, in cases where an electric current is available for lighting.

Chemical batteries may be thought suitable, but in using them for hand electric lights for reading circles and lighting cross-hairs for astronomical work, at Cornell University some two years ago, Professor E. A. Fuertes, M. Am. Soc. C. E., found them so difficult to keep in order that they were abandoned, and a return had to be made to common lamps. With the recent improvement in storage batteries, it is possible that they may soon be relied upon.

The light should be intensified by placing a parabolic reflector behind it, or a spherical reflector behind and a lens in front, so that a large portion of the light would be condensed into a cylinder, or slightly divergent cone of rays directed towards the aligning instrument.

Excellent results, however, are obtained in geodetic work with kerosene lamps ; an 8-inch reflector or less rendering the light capable of bisection at distances of 40 miles, while the lamps are easily managed with unskilled labor. In the "optical collimators" used by the French, a kerosene lamp, with a flat wick, is placed in a box ; two small plano-convex lenses, with their convex sides towards each other, are placed on one side opposite the flame, so as to bring the rays to a focus at the edge of the box ; a spherical mirror is placed on the opposite side, so that the direct rays striking it are reflected back, through the light, to the lenses. To this box is attached a second one, which contains a lens 8 inches in diameter and 24 inches in focal length. The rays of light, which are brought to a focus at the side of the first box, continue as divergent rays, through the second box to the large lens, and emerge as parallel rays. The flame and wick showing flatwise to the lens, the pencils of rays from different points of the light, form a cone with a field of about one-half a degree, instead of a cylinder. A telescope attached to the box, with the line of collimation parallel to the axis of the lens, serves for pointing the collimator to the observing station. The apparatus, as thus described, is over 3 feet long, and weighs about 250 pounds.

For tunnel work, the lens can be reduced to 3 inches, or perhaps less, and the field increased to 1 or 2 degrees. The instrument could then be simplified, the weight reduced, and the pointing made easy without the directing telescope. Some experimenting might be necessary in securing the best arrangement of detail for portability and penetrating power.

H. F. Dunham, M. Am. Soc. C. E.—The writer did not think the details of the work described would lead to so much discussion. It might be mentioned that Mr. W. J. Yoder, Member of the Western Society of Engineers, who had immediate charge of the instrument work referred to, substituted for the plummet and lamp a common lamp placed on a shelf fastened to the back of the gauge board where its light passed through a small opening and also illuminated a spirit level which was permanently attached and used to fix the proper relative position of the gauge. This was more convenient and equally accurate.

While grateful for the attention given to the descriptive part, such details would hardly be thought suitable for a paper had they not offered a ready method of introducing a subject which the writer believed might be of interest, viz., the design and use of an instrument by which fairly accurate results could be obtained quickly and easily in tunnel work. If practicable, it should also be of value in mining and subterranean work.

The mention of the use of lanterns by Mr. O'Rourke, the reference to a search light and the discussion by Mr. Crandall, are pertinent. There might be nothing required differing in principle from that which has been used, but it should be a finer instrument than the search light and capable of producing a beam in which the rays would not diverge so much as a degree.

The power of penetration required might not be a serious obstacle. The small plummet lamp could be seen, except for limited periods, at distances of 800 and 1 000 feet through a telescope having a power of twenty-four, from which one might infer that a lamp of twenty-four times the power of the one used could be seen by the naked eye and without a reflector. From this to a ray powerful enough to produce a light spot on the face of the heading or to be seen in the atmosphere of the tunnel would not seem to be a difficult step.

The writer cannot agree with Mr. Nichols and others that too much stress is laid upon accuracy in tunnel alignment during construction, unless it be assumed that the engineer devotes his energies to bringing his lines and grades together within so many tenths of an inch. In this sense, undoubtedly, their words were used. But let the engineer take a wider view of the work; let him believe that needless labor is, at best, unfortunate labor, although the principal contractor and the laborers are paid for doing it; let him seek to arrive at the best results with greatest economy, and he may find the usual method of giving fixed points at quite long intervals faulty and in the end expensive. Good foremen are remarkably skillful and betray excellent judgment, but they can hardly be expected to keep the two halves of a heading symmetrical for long distances, or to extend grades of wall plates accurately by methods that would be avoided upon the surface. Failing

in this, the arches are unequally loaded, the wall plates are blocked up by a needless amount of material, and change their position as the bench is removed, causing the roof to settle, joints to open, and incidentally making the work of the foreman in producing lines more difficult and uncertain.

The specifications for a timber tunnel in rock, even where masonry is to be introduced when the timber fails, should require for every wall plate a bearing against the solid rock throughout at least one-half the area of its outer side, and the under side should be supported in like manner until the bench is removed. Evidence that such a clause is not too rigid might be found in the average American tunnel, where the writer believes foremen have been too much neglected while good professional results were established.

The aim should be to bring the work of the foreman within closer limits, thereby causing the removal of less material and the replacement of less, and securing greater stability and permanence in the finished work, and, if possible, the work of the engineer should be diminished by employing simpler methods.

These objects were not overlooked when the brief paper under discussion was written, but an extended reference to the advantages of an untried device was naturally avoided.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

557.

(Vol. XXVII.—October, 1892.)

COMBINATION BRIDGE BUILDING ON THE PACIFIC COAST.*

By ALFRED D. OTTEWELL, Esq.

READ OCTOBER 5TH, 1892.

For the purpose of this paper the term "combination bridge" is understood to mean a bridge in which the trusses are made of a combination of wood and iron, or wood and steel. The members of the truss in compression being generally of wood, and the members in tension being generally of iron or steel. This definition is intended to bar out Howe trusses, suspension and arch bridges, for the reason that they are not generally understood when the term "combination bridge" is used. A Howe truss has its tension and compression members in general of wood, although we once in a while hear of an iron Howe truss, in which all its members are composed of iron. In the suspension and arch bridge the stiffening truss is of a secondary nature, and therefore the make-up of that trussing takes second place in the designation of the bridge.

* Discussion on this paper, received before December 15th, 1892, will be published in a subsequent number.

The system of webbing adopted for combination bridges is generally of the Warren, Pratt or Petit form. The Pratt form in ordinary truss bridges being used for spans up to about 200 feet, and the Petit for spans above 200 feet. Multiple intersection webbing has given way to the much superior webbing of Petit. The amount of inclination given to the top chord of ordinary combination truss bridges is one of the vexed questions of the day among engineers on this coast, as it would appear to be in the East. The writer's experience confirms the opinion that for spans up to about 300 feet, parallel chords are preferable between the end hip connections, and for spans above this it is best to deviate from parallel chords as little as economy will allow.

The term "Pacific coast" is intended to refer in general to the States of California, Oregon, and Washington. The timber of the northern portion of this section of the United States having such a world-wide reputation for its quality, it is not surprising that such timber has been adopted on the coast in the construction of bridges in places where it was so well fitted to perform the required duty. That it is extensively used on this coast for purposes for which iron or steel is used in the eastern part of the United States, is due partly to its excellent quality, and partly to the distance of the point of production of the metals mentioned from the Pacific coast. The effect of this distance is augmented by the fact that iron and steel, manufactured in the East for use on this coast, is generally carried continuously by rail overland, usually to be manufactured and consumed at interior points, and carriage by sea round Cape Horn consumes too much time, although it saves considerable expense. That combination bridges may be expected to be seen on the Pacific coast long after they have disappeared from the Eastern States, would seem obvious when it is remembered that the forests of pine in the States of Oregon and Washington are immense, the population of the coast small, the cost of carriage of iron by rail necessarily great, and the probability of the extensive manufacture of bridge iron and steel on the Pacific coast in the near future is small. When Nature provides wood so liberally, there is little inducement to manufacture iron as a substitute.

To get an idea of the saving effected by the use of wood instead of iron, we will consider one 22-foot panel of a top chord subject to a strain of 300,000 pounds. In a combination highway bridge a stick

20 x 20 inches would be used; and in a steel bridge the section would consist, say, of two plates 15 x $\frac{7}{16}$ inches, and four angles 3 x 3 x $\frac{1}{2}$ inches, laced top and bottom, with $2\frac{1}{4}$ x $\frac{3}{8}$-inch lattice bars. The material in the panel of the combination bridge would consist of, say, 734 feet B. M. of Oregon pine, and say 200 pounds of cast iron in the chord-block. The material in the panel of the steel bridge would amount to about 2 500 pounds of steel. Assuming that the cost of material in place in the bridge is as follows :

Lumber	$20 per M B.M.
Steel	$5\frac{1}{2}$ cents per pound.
Cast iron	$4\frac{1}{2}$ " " "

The cost of the steel panel will be	$137 50
And the cost of the combination panel	$23 70
Difference	$113 80

While these figures are of necessity of a very approximate nature, and by no means indicate the relation between the cost of a combination bridge and the cost of a steel bridge, they do serve to indicate what it costs to substitute wood for steel, or steel for wood, in a particular member.

For the reasons given above, the subject of this paper would promise to be contemporaneous for some time to come. No attempt is made to give a history of combination bridge building on the coast, though, indeed, one could be written containing much of interest, experience and no little humor. For the purposes of the paper it is considered advisable to confine illustrations to two examples from recent practice, which, though not the most recent, have already stood tests as severe as any which will probably ever be brought to bear upon them.

The first example is the cantilever bridge across the North Umpqua River, near Roseburgh, Oregon. It is, as far as the writer is aware, the only combination cantilever of large span in existence. The shore arms are each 147 feet, the river arms 105 feet each, and the suspended span 80 feet, making the distance between river piers 290 feet, and the distance between end or anchor piers 584 feet. The bridge, as constructed, illustrates the principle of the cantilever very simply. The river span is connected and supported by the river arms at four points only. The weight of the river span is balanced about the river piers by

the anchor piers or weights at the outer ends of the shore spans. As will be seen, there is no connecting member between the hip panel points of the suspended span and river arms, the wind pressure on the upper chord of the suspended span being transmitted down the end brace to the bottom chord of the cantilever arms to the earth. The lower part of each pier is built of concrete, set on the solid rock of the river bed. The upper part of each pier consists of two iron cylinders, filled with concrete, and braced by wrought-iron horizontal struts and diagonal ties. The bracing is protected from drift by timber sheathing on each side of the bracing. The up-stream cylinder was anchored down to the concrete base by two 1¾-inch galvanized iron rods, to increase the stability against drift. The smallness of the anchor piers is due to the unusual length of the shore arm as compared with the river span.

The method used in providing for expansion and contraction is shown on Plate LVIII. After erection and adjustment the slot in panel point 25 at the fixed end (Plate LVII) of the suspended span was keyed up, so that the whole expansion and contraction of the river arms and suspended span were afterwards taken up by the motion of the pin in the slot (Plate LVIII). As usual in the superstructure of combination bridges, the floor beams, joists, floor and railing are of wood. The compression members are of wood, with the exception of the struts and bottom chord panels next the river piers, which are of steel. The tension members are of iron, and the pins of steel; the chord-blocks, post-shoes, etc., being of cast iron. The shore arms were made of unusual length so as to offer as little obstruction as possible to drift, of which there is considerable in the rainy season.

The method of erecting the suspended span, without false work, by working out from the river piers, was, to some extent, different from the usual method adopted for iron construction, since the compression members in combination work will not in themselves take tension, nor the tension members take compression; nor will any member take transverse loads or shear. For these reasons it was found necessary in the course of erection to introduce several temporary ties and struts.

The second example illustrating this paper, is the bridge across the Salinas River, near Salinas, Monterey County, Cal., hereafter referred to as the Hilltown Bridge. It consists of two Petit truss spans of 298 feet 1 inch each, on pile piers. The center pier consists of thirty-four redwood piles, and the end piers of twenty-six piles. Two sites were

proposed for this bridge, indicated on the plan as the upper and lower crossing. The bed of the river at both these crossings consists of quicksand, gravel, and clay, to a great depth. The lower crossing was adopted for the reasons that it was shorter than the upper, and the banks at the lower offered more resistance to a change of course in the river than at the upper. The piles for the pier were of hewn redwood and not less than 10 inches diameter at the point, driven as far as practicable without injury to the pile. The piles were efficiently braced and capped, as shown, and sheathed with redwood from low water to the top of the pier. The truss-timbers, floor-beams, floor-joists, floor-planks, and railing were of Oregon pine.

The whole of the iron and steel work for these bridges was manufactured by iron works on this coast, both bridges being designed, constructed and erected by the California Bridge Company in 1888–89.

List of Photographs illustrating this paper :

List of Drawings illustrating this paper :

PLATE LII.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 557.
OTTEWELL ON COMBINATION BRIDGES.

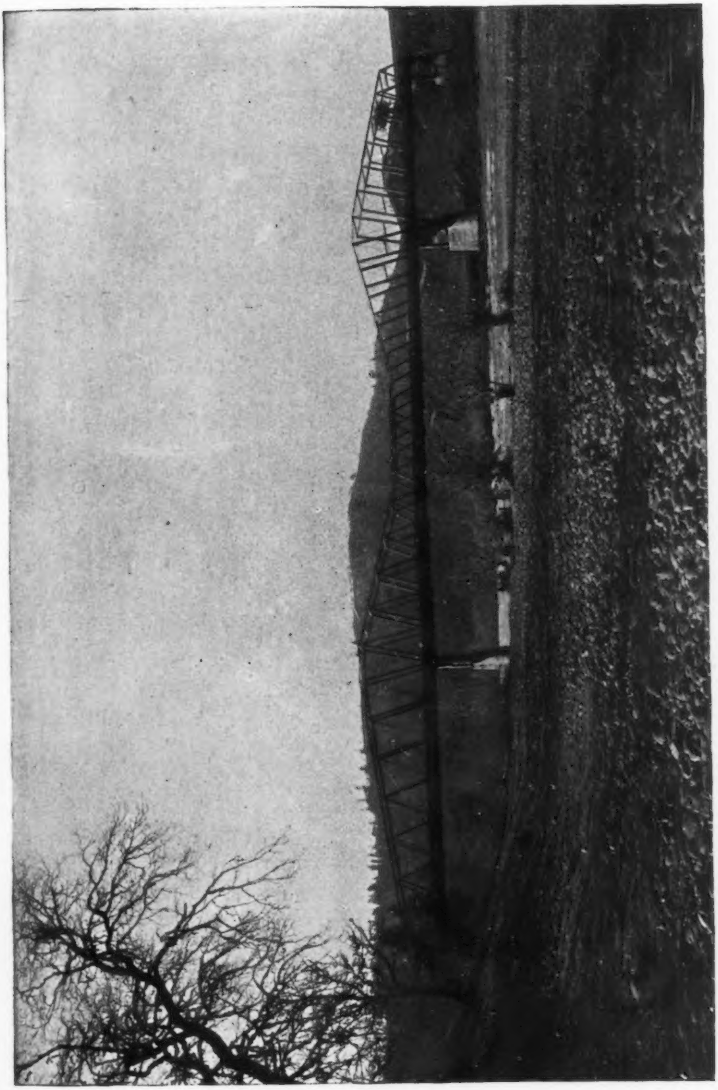


PLATE LIII.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 557.
OTTEWELL ON COMBINATION BRIDGES.

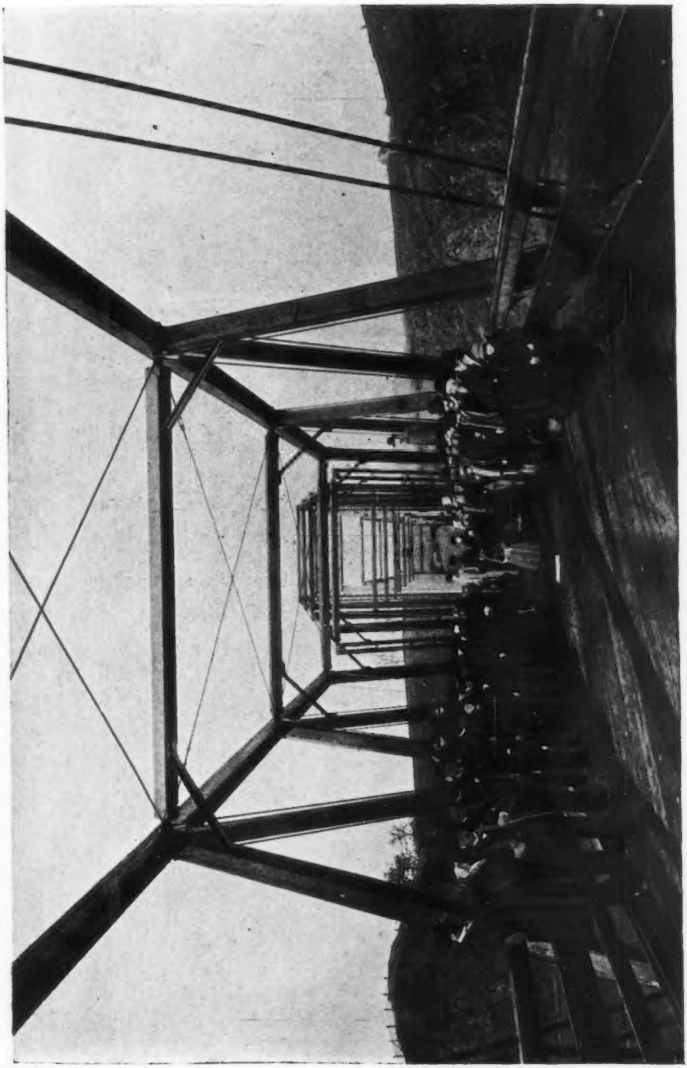
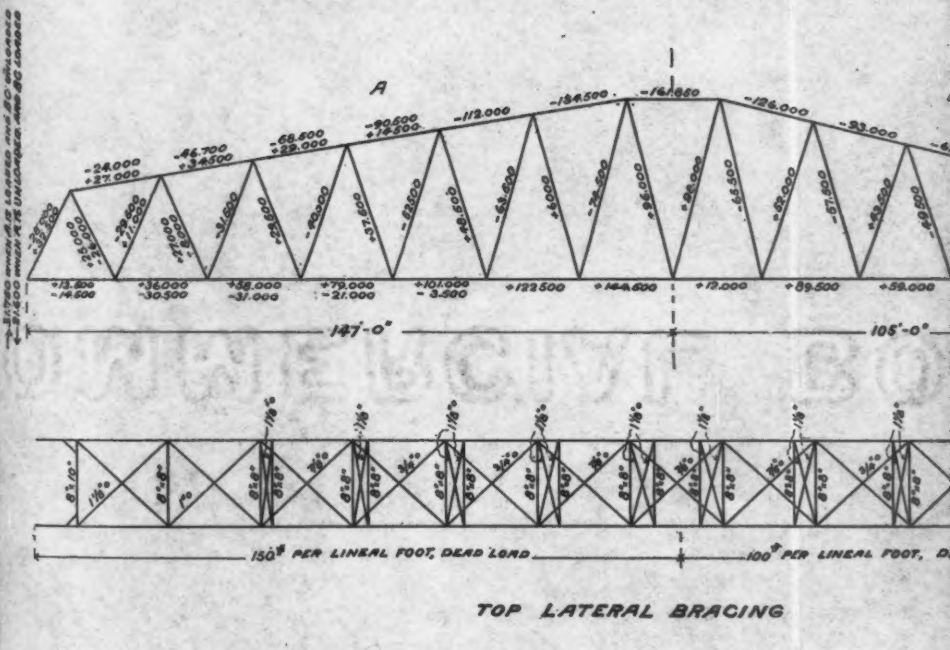


PLATE LIV.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 557.
OTTEWELL ON COMBINATION BRIDGES.

→ 31.760 WHEN A IS LOADED AND BC UNLOADED
← 31.600 WHEN A IS UNLOADED AND BC LOADED



TOP LATERAL BRACING

LIVE LOAD 800# PER LINEAL FOOT

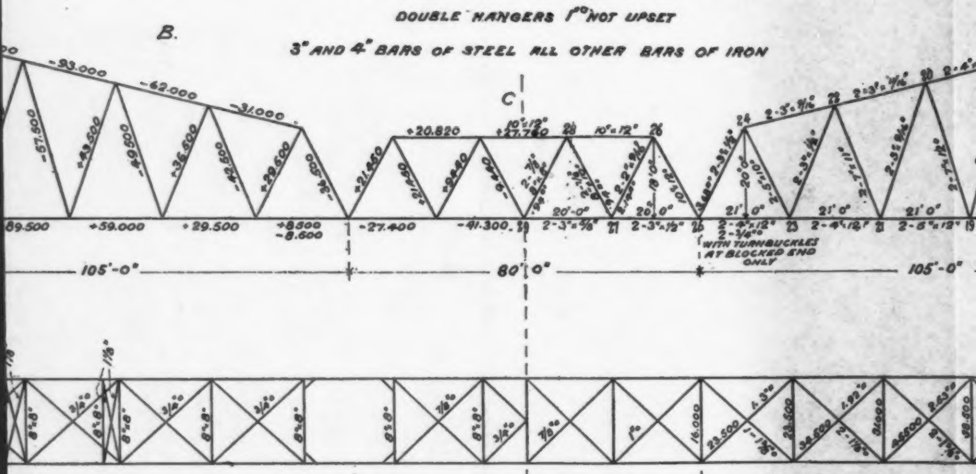
AVERAGE DEAD " 800# " " "

TENSION ON STEEL CHORDS AND MAIN TIES 15.000# PER SQ. IN

" " COUNTERS AND BEAM HANGERS 10.000# " " "

DOUBLE HANGERS 1" NOT UPSET

3" AND 4" BARS OF STEEL ALL OTHER BARS OF IRON



R LINEAL FOOT, DEAD LOAD
100# PER LIN. FT. DEAD L'D. 200# PER LIN. FT. LIVE L'D.
100# + 150# = 250# PER LIN FT DEA
100# 100# 350# " " LIVE

ALLOWABLE UNIT STRESS 18000# PER SQ. IN.

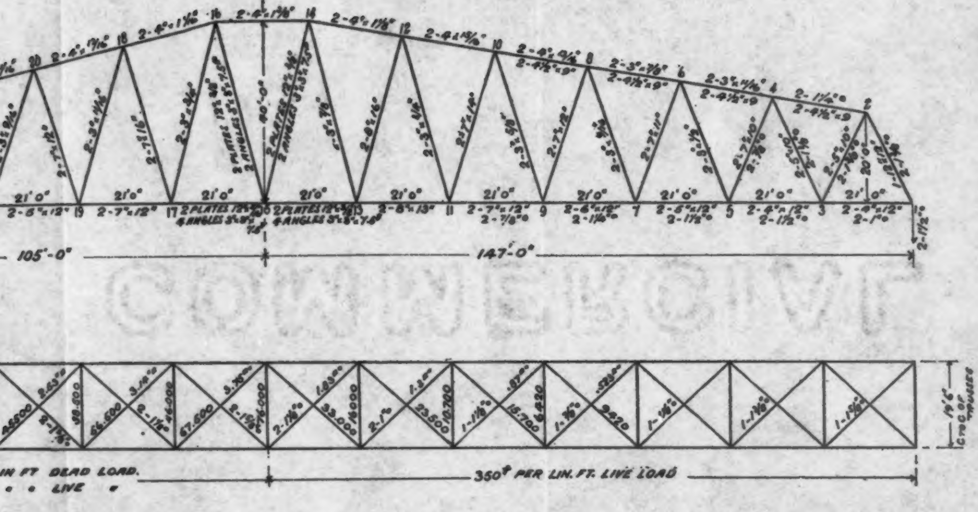
BO

PLATE LV.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 557.
OTTEWELL ON COMBINATION BRIDGES.

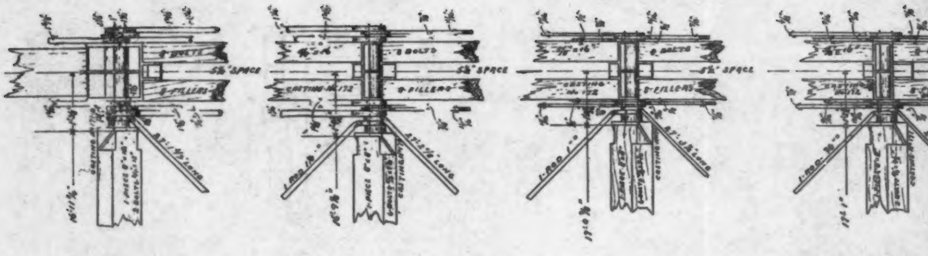
STRAIN SHEET FOR CANTILEVER BRIDGE

OVER NORTH UMPQUA RIVER

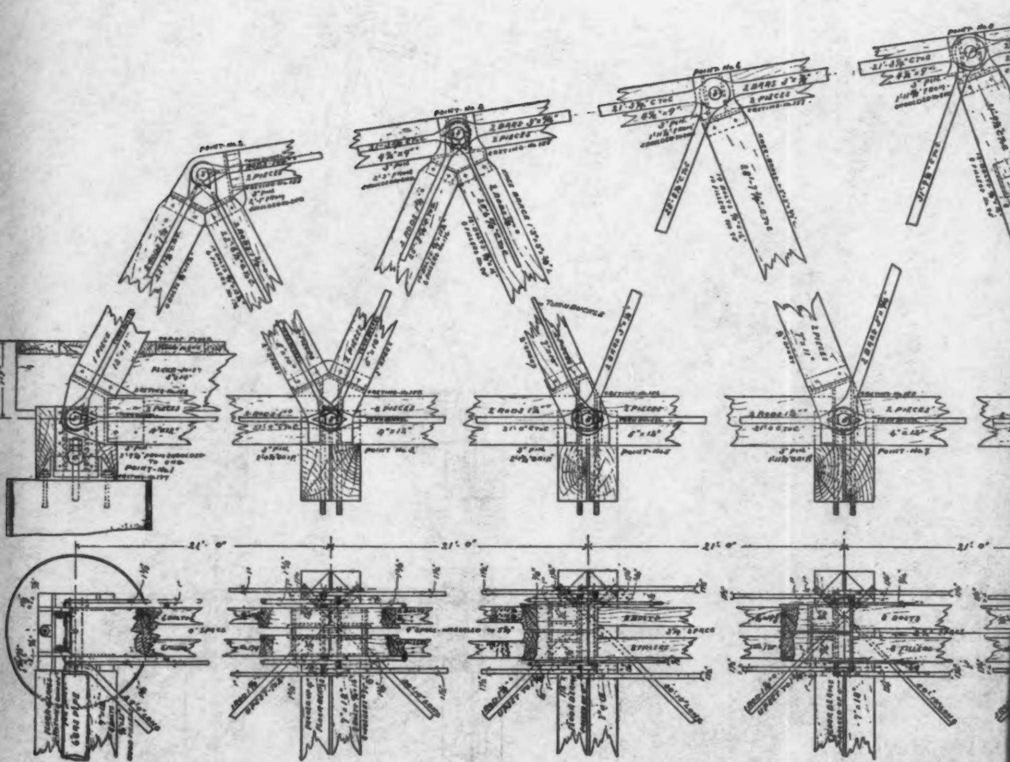
DOUGLAS COUNTY OR.



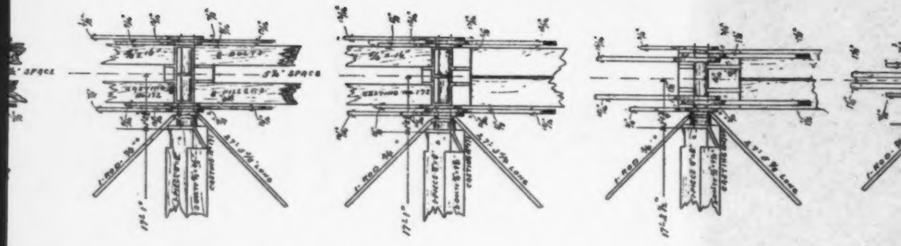
BOTTOM LATERAL BRACING.



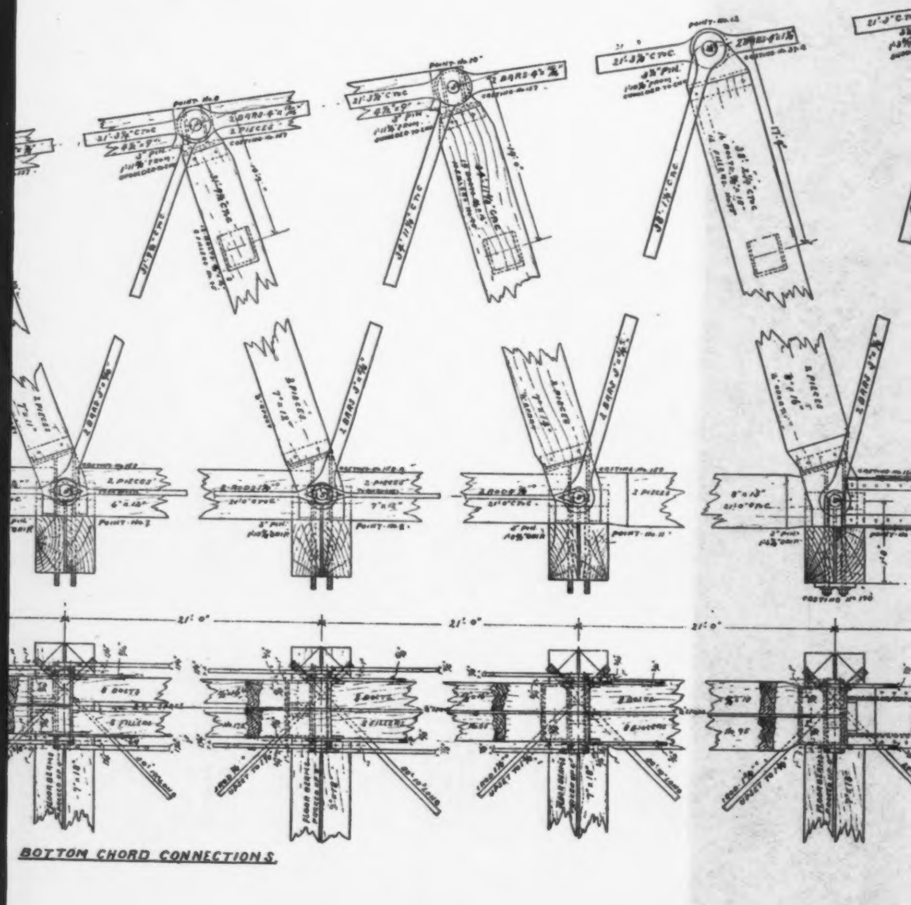
TOP CHORD CONNEC



BOTTOM CHORD CONNECT

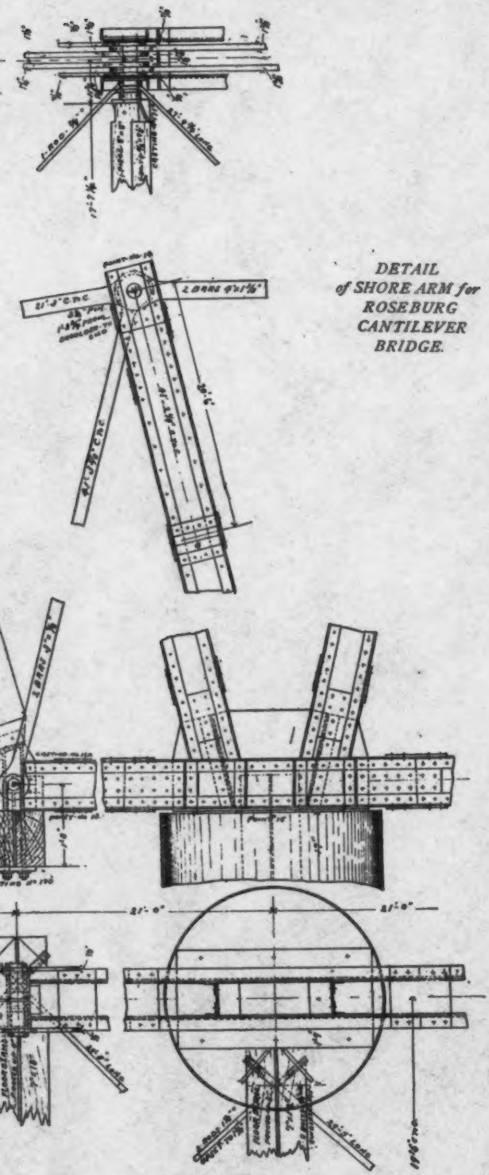


TOP CHORD CONNECTIONS.

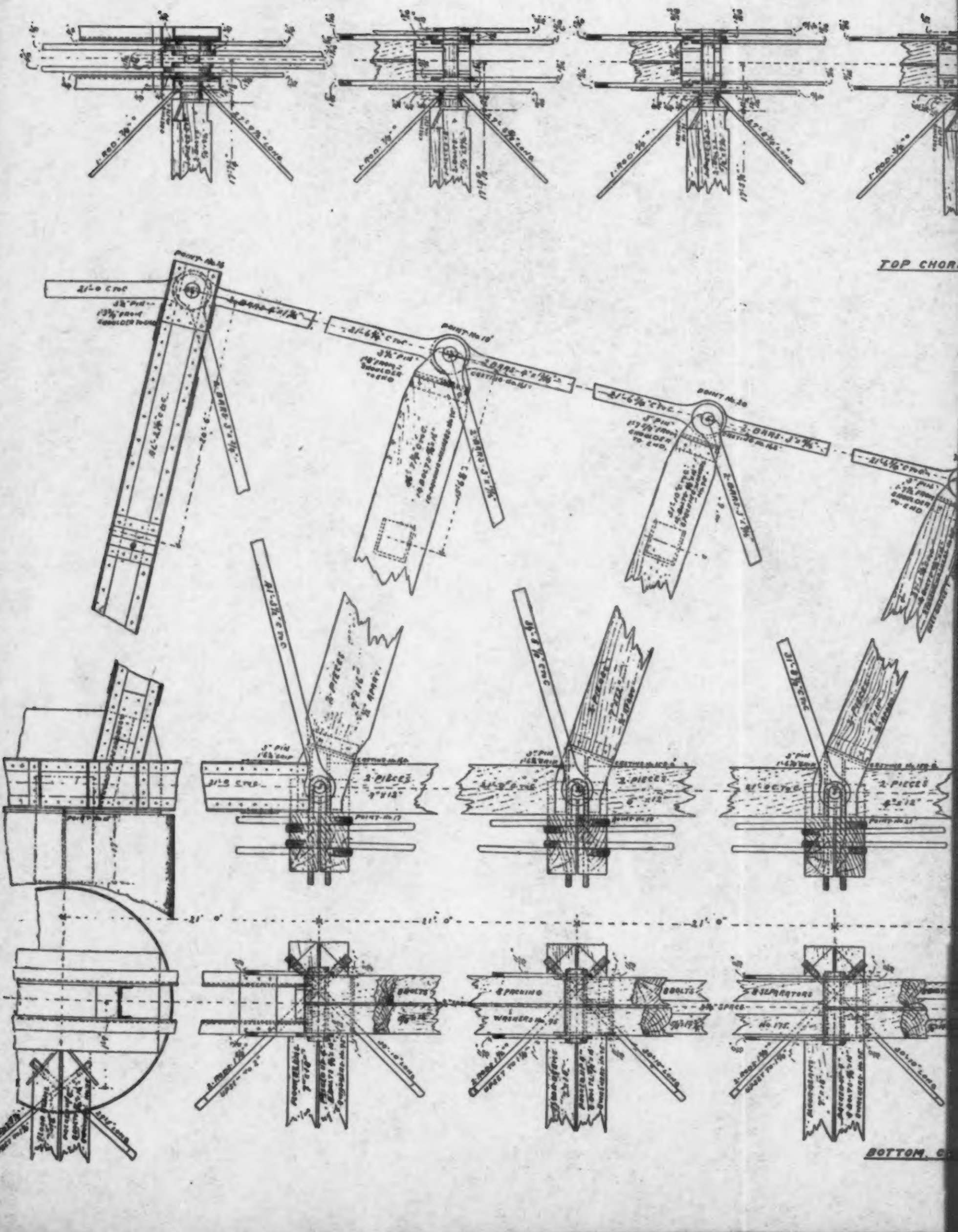


BOTTOM CHORD CONNECTIONS.

PLATE LVI.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 557.
OTTEWELL ON COMBINATION BRIDGES.



DETAIL of SHORE ARM for ROSEBURG CANTILEVER BRIDGE.


TOP CHOR
BOTTOM C

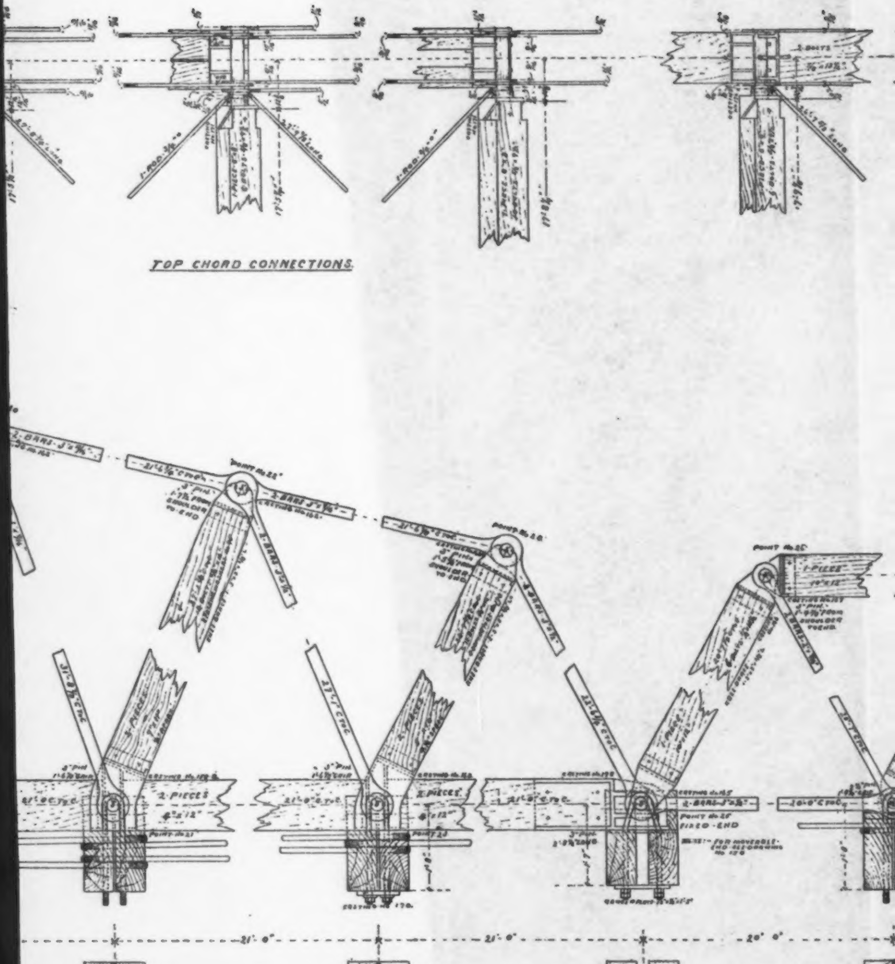
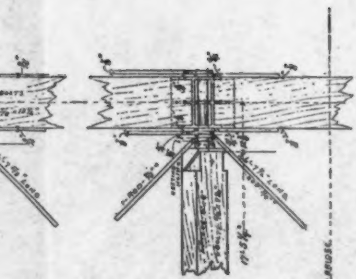
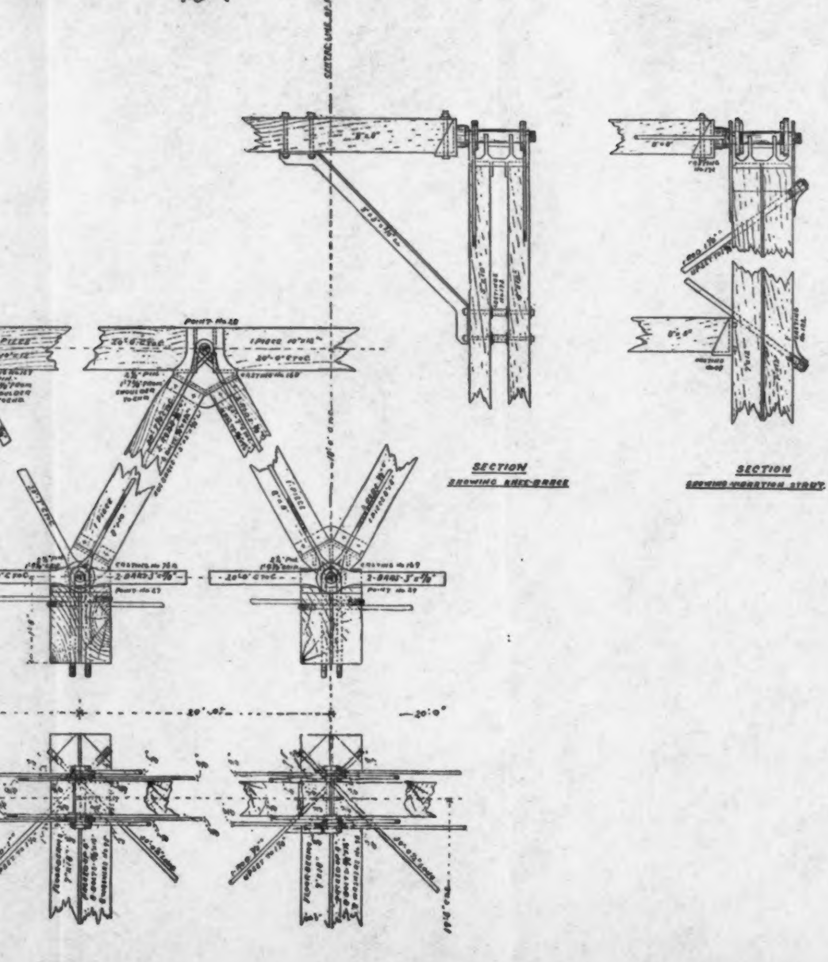

TOP CHORD CONNECTIONS.
BOTTOM CHORD CONNECTIONS.

PLATE LVII.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 557.
OTTEWELL ON COMBINATION BRIDGES.

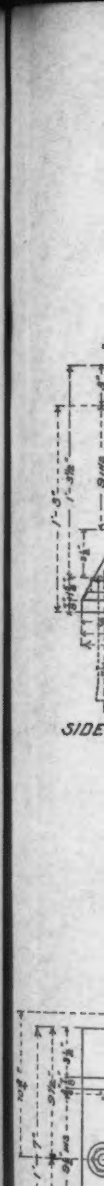
DETAIL
of RIVER ARM for ROSEBURG CANTILEVER BRIDGE

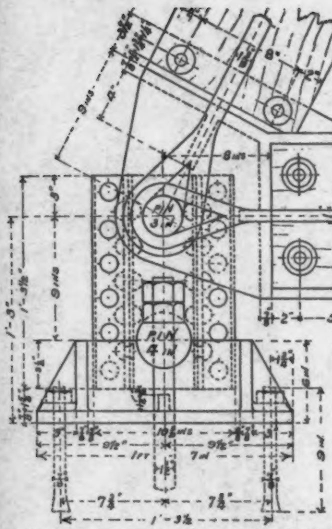




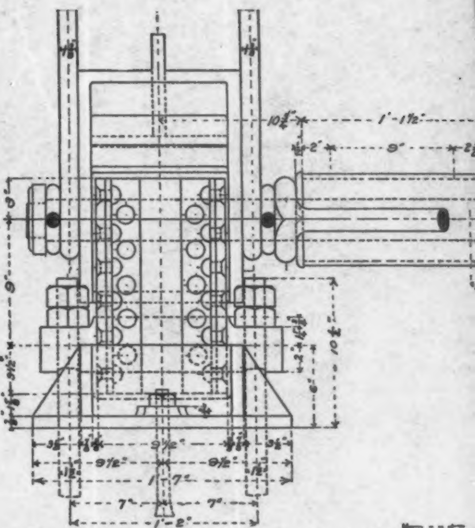
SECTION SHOWING KNEE-BRACE

SECTION SHOWING ...... STRUT.

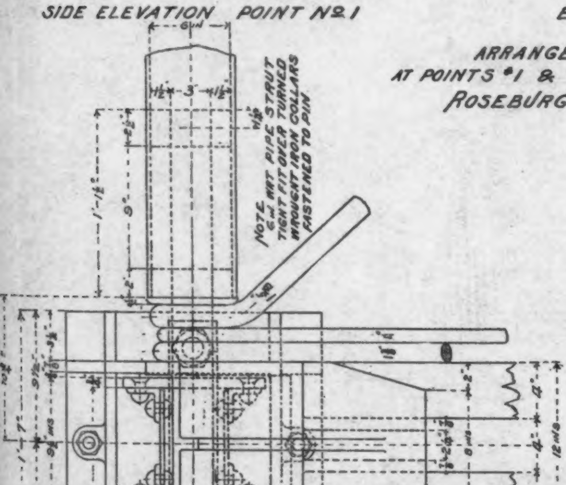

SIDE



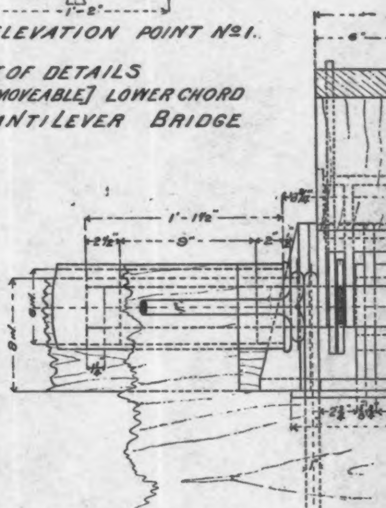
SIDE ELEVATION POINT No. 1


END ELEVATION POINT No. 1.

ARRANGEMENT OF DETAILS
AT POINTS #1 & #25 [MOVEABLE] LOWER CHORD
ROSEBURG CANTILEVER BRIDGE



PLAN POINT No. 1.


END ELEVATION POINT No

PLA
TRANS. AM.
VOL. XX
OTTEWELL ON CO

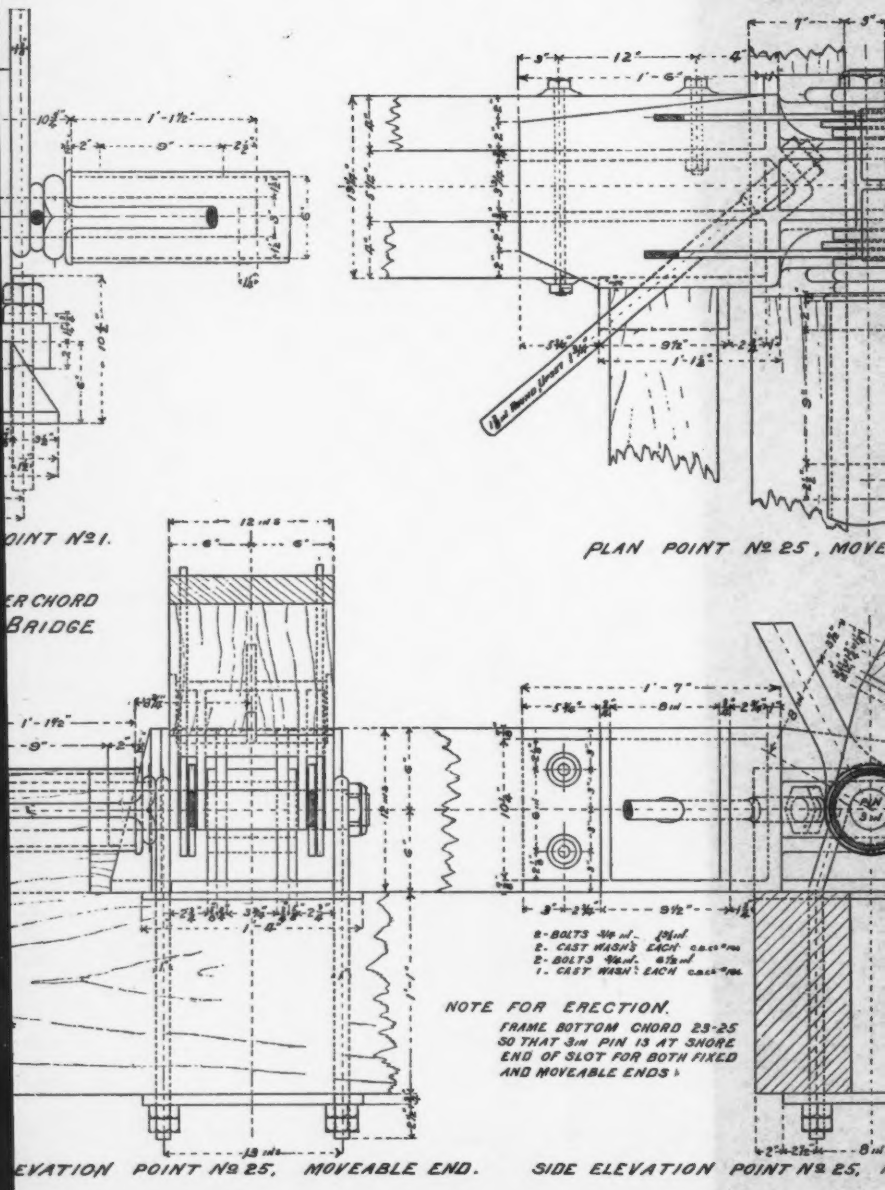
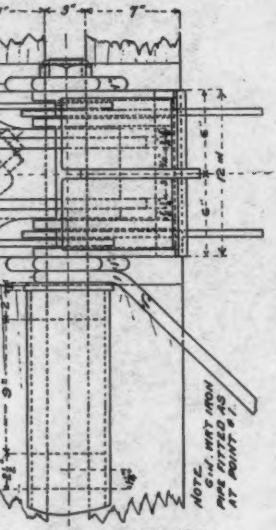
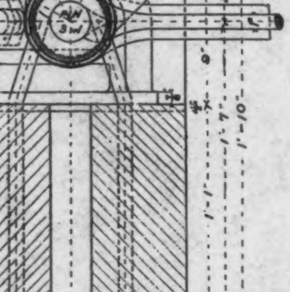


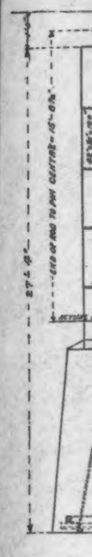
PLATE LVIII.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 557.
WELL ON COMBINATION BRIDGES.

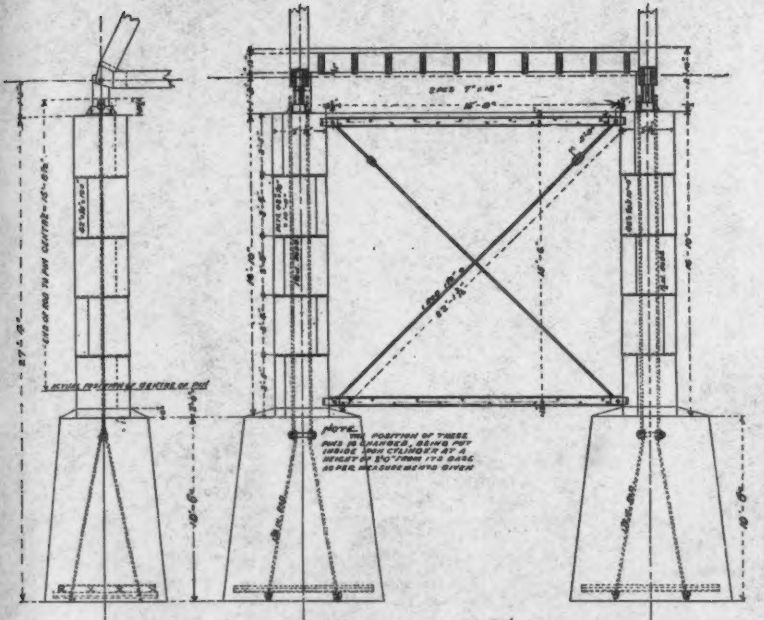


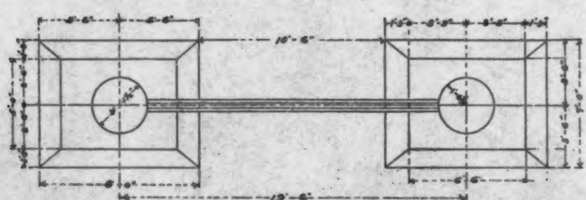
S, MOVEABLE END



Nº 25, MOVEABLE END

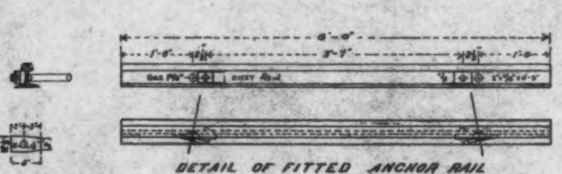




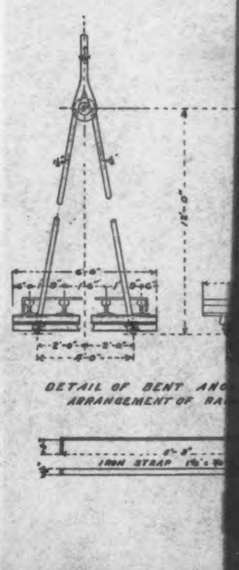


BILL OF MATERIAL FOR 2 SHORE PIERS

4	CYLINDERS		15'-10"	HIGH
8	CHANNELS	6 in 12.#	16'-4"	STRUTS
4	RODS	1¼ in Ø	22'-1⅜"	DIAGNL.
8	"	1½ " "	15'-8½"	ANCHOR
8	"	1⅜ " "	25'-5"	"
16	"	¾ " "	2'-5"	"
8	RODS	2½ " "	0'-7⅜"	PINS
4	"	3" "	1'-6½	"
16	PLATES	6 in × ⅝ ×	1'-2½	HITCHES
8	"	4½ × ⅝ ×	1'-7"	REINFORCE
8	"	6" × ½ ×	1'-0"	ANCHOR
16	COTTERS	⅝ Ø ×	0'-3½	PIN KEYS
56	WRT. I. PIPE	¾ in −	0'-3½"	THIMBLES
8	RAILS		6'-0"	ANCHOR
16	"		8'-0"	"
16	PLS.	2 × ¼ ×	0'-8"	RAIL HITCH.
16	"	2½ × ¼ ×	0'-5"	" WSHS.
90	RIVETS	⅝ in −	2¼	FIELD
12	BARS	1½ × ¼ ×	5'-0"	STRAPS
4	W.I. PIPES	4" DIA.	10'	[illegible]

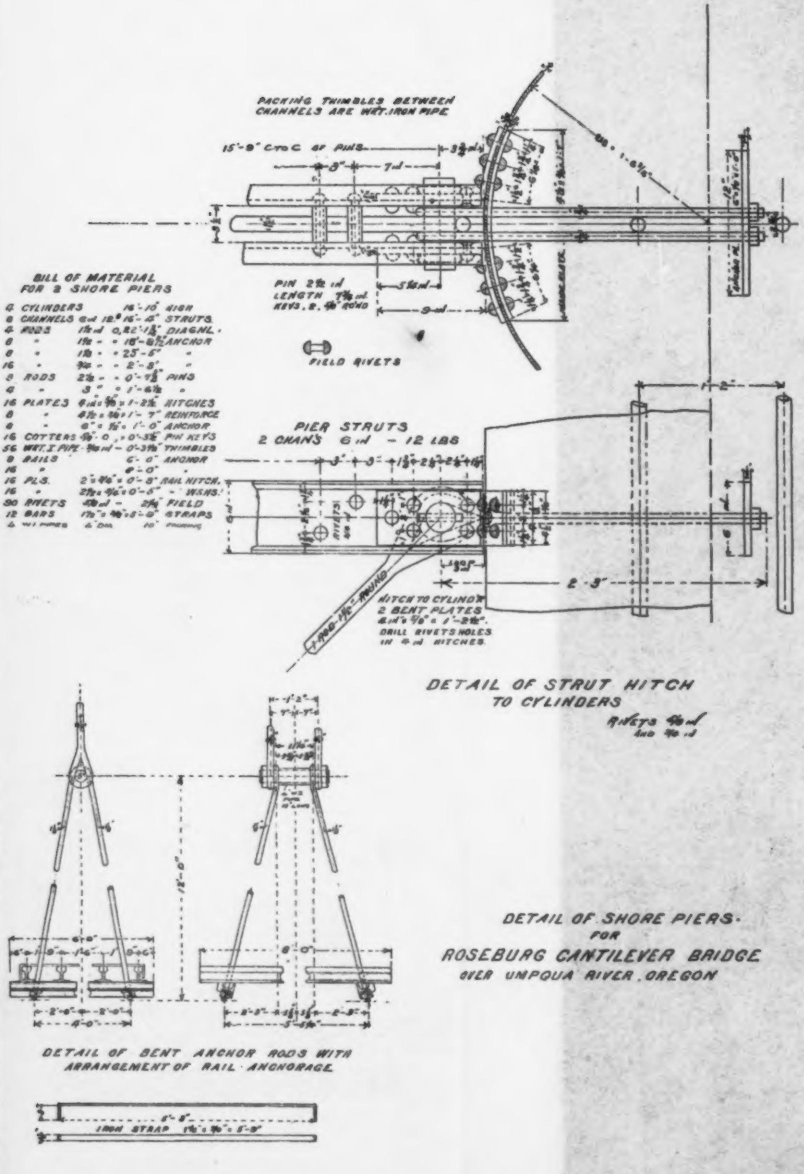


DETAIL OF FITTED ANCHOR RAIL

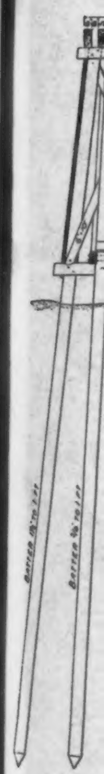


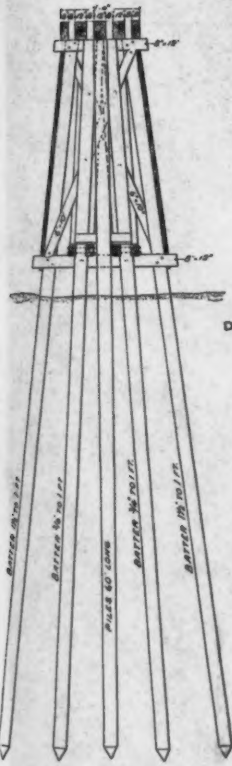
DETAIL OF BENT ANC[...]
ARRANGEMENT OF RA[...]

PLATE LIX.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVII, No. 557.
OTTEWELL ON COMBINATION BRIDGES.

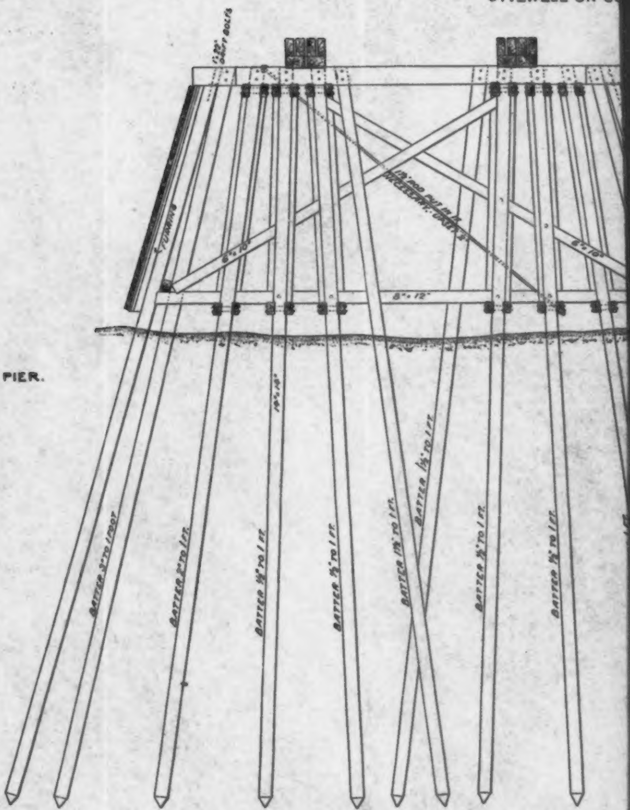


ES.





DETAIL OF 34 PILE PIER.



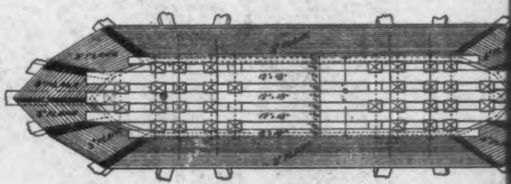


PLATE LX.
TRANS. AM. SOC. C
VOL. XXVII, No.
OTTEWELL ON COMBINA

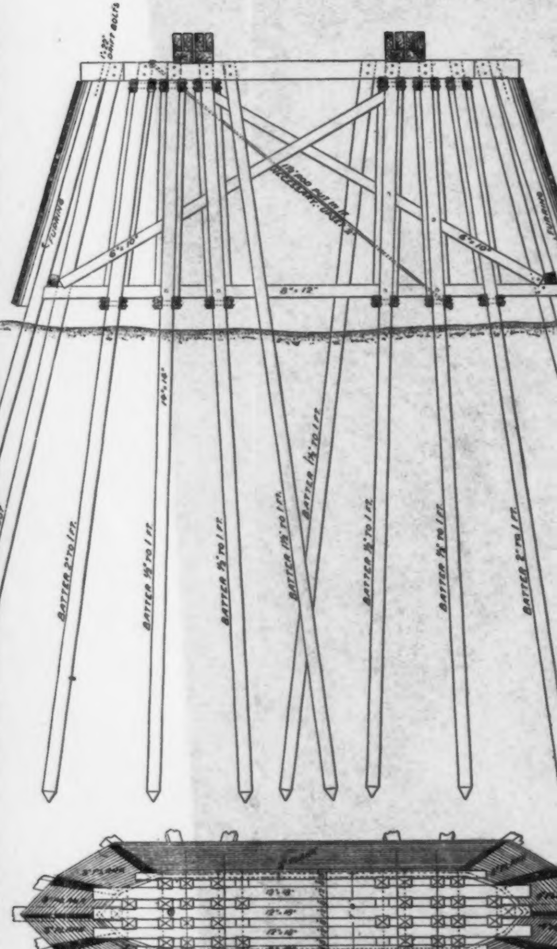
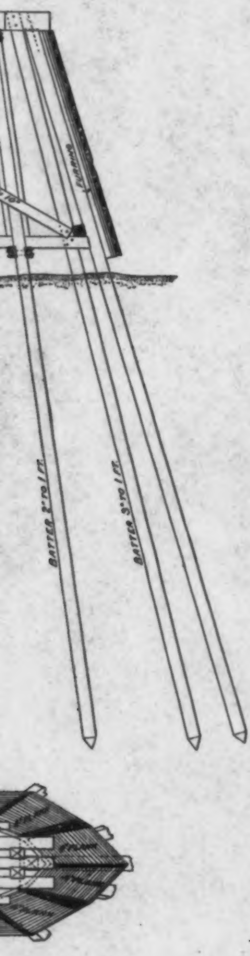
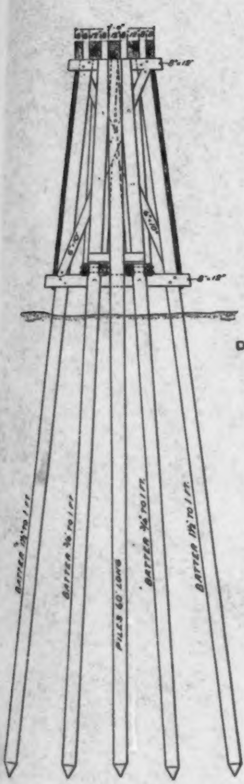


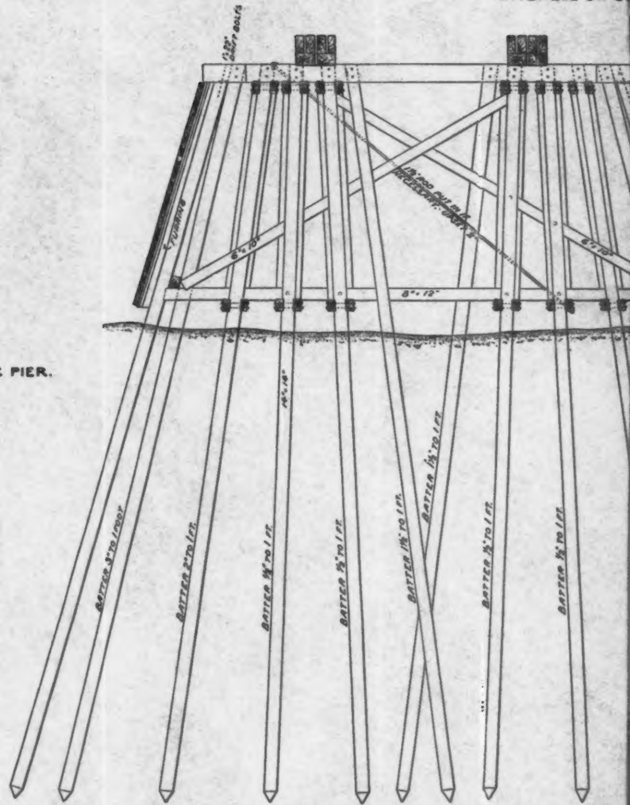
PLATE LX.
AM. SOC. CIV. ENGS.
L. XXVII, No. 557.
ON COMBINATION BRIDGES.



PL
TRANS. AM.
VOL. XX
OTTEWELL ON C



DETAIL OF 34 PILE PIER.



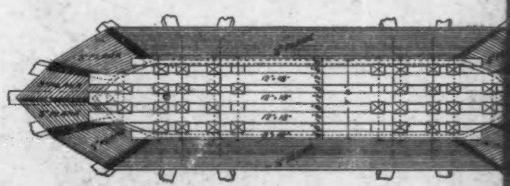
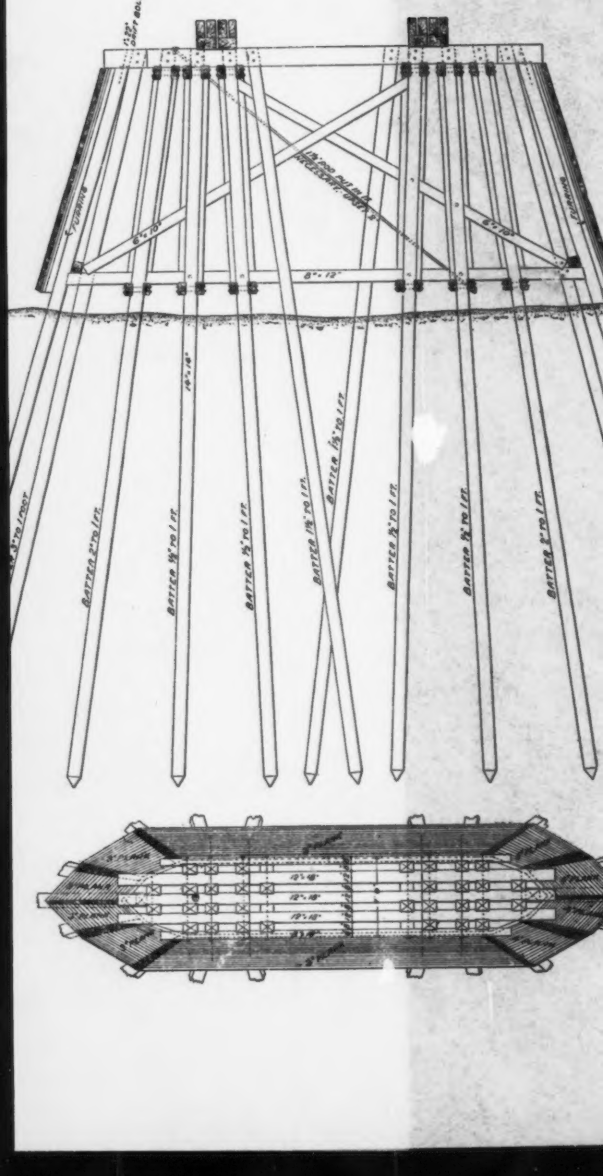
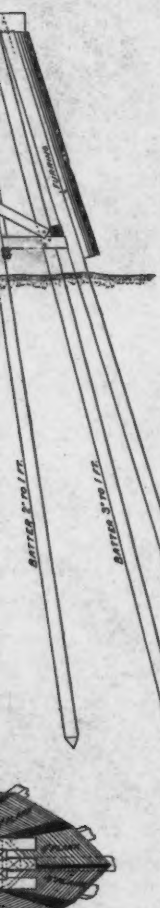


PLATE LX.
TRANS. AM. SOC. CIV.
VOL. XXVII, No. 55
OTTEWELL ON COMBINATIO



LATE LX.
. SOC. CIV. ENGS.
XXVII, No. 557.
COMBINATION BRIDGES.



AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.
NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

558.
(Vol. XXVII.—October, 1892.)

SOME NOTES ON FOUNDATION EXPERIENCES.*

By A. P. Boller, M. Am. Soc. C. E.

Read October 5th, 1892.

GAS HOLDER TANKS, BAY STATE GAS COMPANY.

The plant of the Bay State Gas Company, built in 1886, is located near the pumping station of the Main Drainage Works of the City of Boston, on Dorchester Bay, and on original marsh land flooded at high tides from the bay. It was designed by the late Joseph Flannery, a leading gas engineer of Philadelphia. The gas holder tanks of this plant about to be described, and to which the writer's relation was simply that of a contractor, are interesting from their magnitude, the speed with which they were constructed, and the manner in which the work was carried out. There are two tanks built of brick, about 30 feet apart, each having an inside clear diameter of 152 feet, with foundation footings sunk about 30 feet below the level of the marsh. The leading

* Discussion on this paper, received before December 15th, 1892, will be published in a subsequent number.

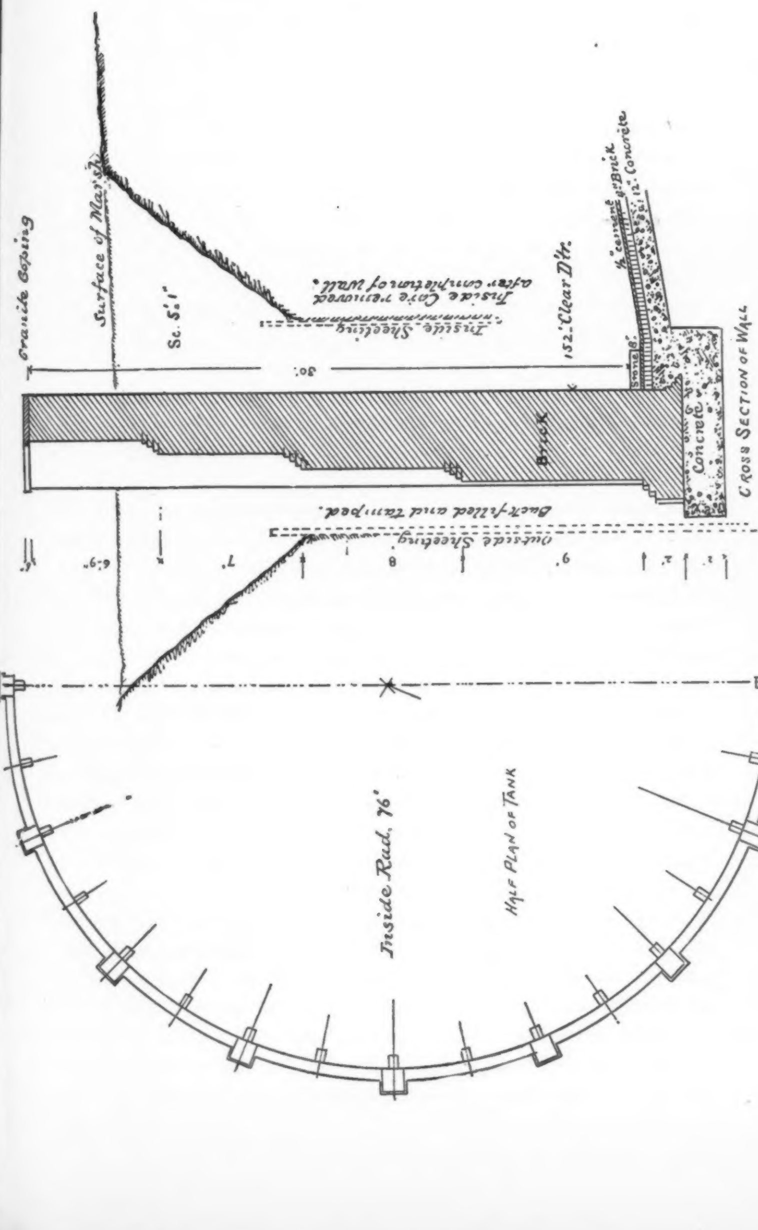
dimensions and arrangement of column piers and roof supports are shown upon the drawing. They were built between the first of June and Christmas of the same year, except the coping (of granite) on No. 2 tank, which severe weather delayed until the following spring.

By the terms of the contract, the contractors were to do their own engineering and inspection under the plans furnished, the principal requirement being that the tanks should be guaranteed water tight and true to circle. The material used was hard burnt Eastern brick, laid in improved Union cement, and coarse gravel concrete made with Portland cement.

The physical conditions under which the tanks were to be constructed were rather forbidding, particularly in view of the difficulties met in securing the foundations for the pumping station of the drainage works some 500 feet away from the site of the tanks. No borings had been taken, it being assumed that the material to be gone through was substantially of the same character as that met with at the pumping station, starting with the marsh mud, stiffening up as clay, and reaching the sand, which was strongly water bearing as would be expected. The enormous area occupied by each tank dictated large pumping capacity.

The mode of construction adopted was as follows: The site of the tanks and construction plant, covering some two acres, was first diked off from the sea, which flooded the marsh at high tide. Immediately between the tanks a sump well 10 feet square was sunk and planked up, into which all drainage was to be carried. The pumping plant consisted of two 80 horse-power locomotive boilers, and four Andrews centrifugal pumps, two 6-inch and two 8-inch discharge, forming a duplicate plant, it being expected that one of each size pump would be amply sufficient to care for the water under the plan of construction adopted, the other set being a relay in case of accident.

In sinking the sump, much difficulty was experienced about two-thirds the way down, from marsh gas; so much so that several workmen narrowly escaped with their lives, and all those who had come under its influence suffered from inflamed eyes for several days after withdrawing from the pit. It was only after a blower had been rigged up for forcing air down the pit and thus diluting and dissipating the gas, that work could be resumed and the bottom of the sump reached and planked with lateral connections to the tank. The walls of the



TANK FOR TELESCOPIC HOLDER.—BAY STATE GAS COMPANY, BOSTON, MASS.

tanks were built in excavations precisely as if a sewer was intended, the interior core being left for excavation after the completion of the walls. The wall excavation was sloped about 1 to 1 for 10 feet, when sheet piling was introduced, 4 inches (tongued and grooved) for the outside row and 2 inches for the inner row.

The sheeting in single lengths was mauled down as far as possible, when a small steam pile-driver was mounted and the sheeting driven clear down below the concrete footing. The driver was a small machine consisting of a single square timber mast on the apex of a triangular base, the hammer of about 500 pounds grooved to slide over a tee-rail guide secured to the mast. It was a very effective machine and easily handled.

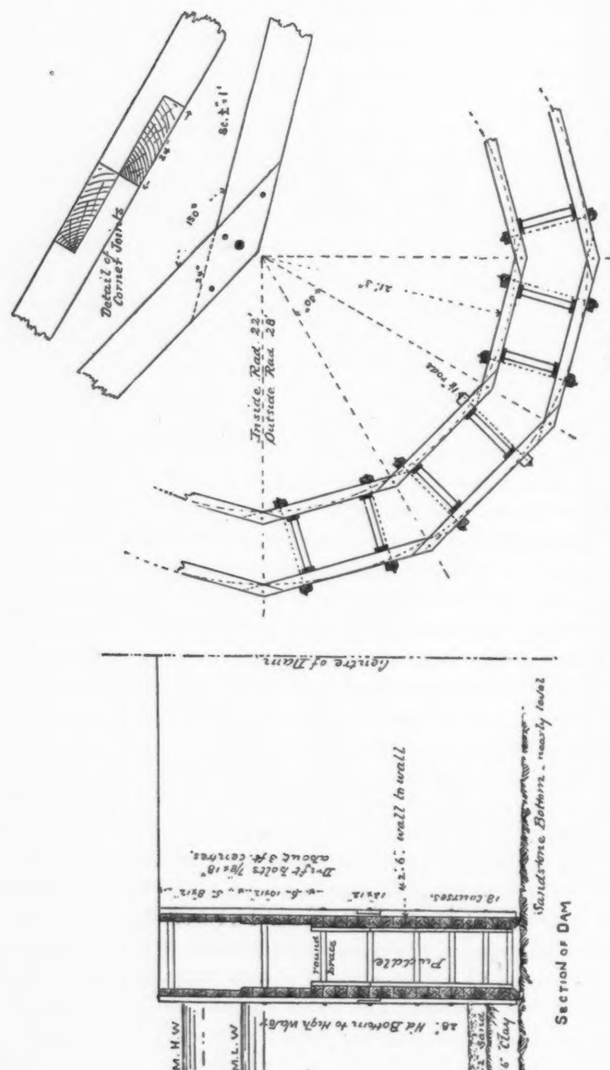
This manner of putting in the tank walls required only a narrow excavation easily braced, reducing the demands on the pumps to a minimum. The excavations were carried on either way from the sump drains, and in sections of greater or less length as contention with water demanded. In building the brick work, to secure tightness, the bond was broken as irregularly as possible in the hearting of the wall (water abhors an angle), and to secure a thorough filling of the joints with mortar, the bricklayers were instructed to keep their fingers over the bricks and lay them a finger width apart. Each course of brick work was thoroughly grouted as the work progressed. The result was an exceedingly tight wall, as will be seen by the record hereafter given, As the wall was built up, the outside void formed by excavation was refilled with the excavated material and well rammed. The circular alignment was maintained, and verticality insured by frequent checks with a standardized steel ribbon revolved around a carefully maintained center hub and plumbing down from the end of the radius. This resulted in such accurate work, that little or no furring was required for setting the gas holder guides, and a trifling amount of cutting into the brick was required in only a few cases.

On the completion of the walls, the central core was rapidly excavated by means of three or four derricks handling yard buckets of material. Little or no difficulty was experienced in putting in the concrete bottom, excepting for about one-third the length near the footings of one of the tanks, which was conquered section by section with sheeting, and boxing in a continuous drain below the concrete. Contrary to expectations, the water difficulty was not great, and bar-

ring a short section above noted in the first tank, where sand was struck, all the water met required only intermittent pumping. Almost the entire excavation, after passing the marsh mud, was in clay, and in the westerly tank no sand whatever was encountered, consequently little or no water was met on the completion of the work. The tanks were filled to their capacity, and the engineer, Mr. Flannery, reported as follows to his company: * * * "The record of the two gas tanks kept from the 9th instant indicate that in No. 1 no appreciable loss has obtained; that in No. 2 a loss of $4\frac{1}{3}$ inches has obtained in each twenty-four hours, being from June 30th, 1.30 P. M., to 1.30 P. M. of the 11th instant, 5 inches; 11th to 12th instant, $4\frac{1}{2}$ inches; 12th to 13th instant, 4 inches; which shows a constant betterment. That these tanks made so excellent a showing at this period of their history is unprecedented. No. 2 shows a rate of improvement which indicates an entire stoppage in the unaccounted-for water in a few days. The evaporation at this season amounts to $\frac{3}{10}$ of an inch in twenty-four hours." * * *

Coffer-Dam of the Center Pier of the Arthur Kill Bridge.

The center pier of the Arthur Kill Bridge, uniting the Jersey shore with Staten Island at Elizabethport, is founded upon the red sandstone of the district, and was built within a coffer-dam of somewhat novel construction, with a view of avoiding all interior bracing, which interferes greatly with rapid and economical building of masonry. The tides run very rapidly in the Kills, rendering it almost impossible for a single oarsman to make headway against them. The traffic through the Kills is of enormous proportions, consisting largely of tows, which, in a comparatively narrow channel, were often forced by wind and tide in close proximity to the center pier. Any coffer-dam had, therefore, to be built within the narrowest limits. The physical conditions at the site of this pier were a nearly level rock bottom (the sandstone being in its natural bed, with not over 10 inches pitch to the eastward in the width of the dam), overlaid with about 2 feet of clay under some 18 inches of sand and mud, and a depth of water over the rock of 28 feet at high tide. The plan of dam adopted was a double-walled polygon of twelve sides, the walls being 4 feet apart in the clear, within which the puddle was placed. The inscribed circle of the inner wall, measuring the free working space, was 42 feet 6 inches in diameter. The walls of the dam were built up of square hemlock timbers as shown, halved into each other at their intersections, and proportioned under the consideration of a horizontal



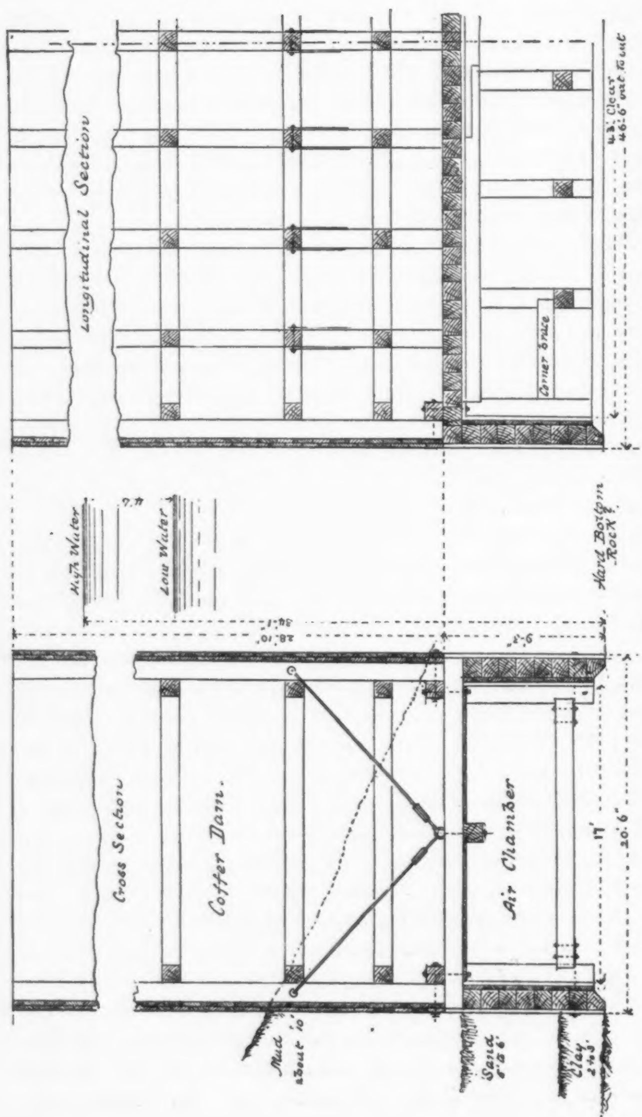
ARTHUR KILL BRIDGE.—COFFER-DAM OF PIER No. 3 FOR DRAW SPAN.

polygonal ring, subject to a uniform load of water due to the head at any point. The separate walls were tied together by bolts and round struts (barked pieces of piles) at intervals, the round struts being adopted to allow the puddle to freely run around them as shoveled in. The bolts passed through clamp timbers of yellow pine 6 x 12 inches, the whole depth of the dam. For convenience of building, these clamps were scarfed in two lengths. The interior struts butted up against pieces of 6-inch plank, so as to catch all the wall timbers.

The dam was built on the shore in launching ways for about one-third its height, when it was launched, and towed to its location and was built up until grounded in position. Between each course of timber and at the scarfing of the joints, a line of cotton wicking was introduced, which by swelling would aid tightness of the walls, and prevent the puddle seeking, under a strong head of water, a vent caused by unevenness of the timber, which events proved to be a wise precaution. The courses of timber were drift bolted every 3 feet with $\frac{7}{8}$ x 18-inch bolts, and the scarfed corners were additionally fastened with 10 x $\frac{5}{8}$ spikes.

Previous to launching the dam, the site of the pier had been prepared by dredging the rock bare, and settling in place the crib blocks on either side, constituting part of the permanent fenders, which are all crib work. The dam, 56 feet across, projected into the channel beyond the fenders $3\frac{1}{2}$ feet on either side. Fortunately it was not struck by passing vessels or tows, although there were some narrow escapes from the latter.

The dam was settled in position and held in place by piling on some of the stone which was to be built in the pier, when the puddle was filled in between the walls. This puddle was a very hard gravelly clay, from a nearby bank, exceedingly difficult to dig, but which packed splendidly under the water as it was thrown in, and made an ideal material for the purpose. Before pumping out, the bottom was prepared by depositing under water a rich Portland cement concrete, containing three barrels of cement and nine barrels of sand to the yard of stone. It was deposited from triangular buckets of one yard capacity and placed by divers, until there was 4 feet of concrete all over the dam. After allowing the concrete to harden for about a week, the dam was pumped out in a few hours for the masons to start their work, and a beautifully tight dam it was, with one exception, and that was a very



ARTHUR KILL BRIDGE.—CAISSON AND COFFER-DAM, PIER No. 4.

small area about 5 feet from one of the walls, where the divers had omitted to properly cover the bottom with concrete, and quite a lively spring spouted up from the bottom. We always believed this to be from a fissure in the rock, and not from under the dam, as the outside edge of the dam had been carefully gone over, foot by foot, by the diver, who bagged with clay any suspicious place, besides which the splendid character of the puddle, so well boxed in, seemed to insure a perfect luting with the rock. As the spring refused to be stopped, it was boxed in and led to the sump, the concrete leveled off and the masonry started, and the box built into the pier.

There was some groaning of the timbers as they settled to their bearings, and quite a perceptible bend to the lower timbers of the inside wall. The tie bolts drew nearly half way into the clamp timbers, a couple of which split up at the scarf joint. The seams between the wall timbers were remarkably tight, thanks to the cotton wicking, although in a few places when the irregular scantling made too wide a joint the cotton leached out, followed by some clay, but such leaks were quickly stopped by some pieces of plank spiked over the seams and calked. The 6-inch centrifugal pump took care of the drainage with perfect ease, and was only run intermittently. Had it not been for the bottom spring before described, a 3-inch pump would have been amply sufficient to take care of all seepage or wall leakings. The only improvement that could have been made in this dam, would have been the use of 10-inch instead of 6-inch clamp timbers, and larger bolt washers; and the author thinks a direct miter joint of the wall timbers instead of scarfing would have made better bearings, but in that case it would have been difficult to have made a unit of the dam for launching, handling and placing, and would have required iron clamps or some device for holding the courses together.

This dam required 140 M. B. M. of timber, 15 000 pounds of iron, and 600 yards of puddle, and is believed to be, for the area embraced and depth of water, as economical and safe a dam as was ever built, to say nothing of the immense gain from freedom of intermediate cross bracing.

Arthur Kill Bridge. Pier No. 4.

This pier, near the Staten Island side, was almost in the sweep of the tows, and putting in the foundations was looked forward to with no little anxiety. It was necessary to have the stoutest kind of fender

guards (which were to be permanent), and a system of oak piling and bracing was devised and put in place as soon as the ice enabled us to get a machine in place and float in material. At this point, to hard-pan bottom from high water was 34 feet; the bottom being overlaid with some 3 feet of clay and 6 feet of sand, with an irregular mud line on top, sloping shoreward from 9 to 10 feet.

It was the original intention to dredge to hard bottom and put in a double walled squared timber coffer-dam, similar in construction to that used for the center pier previously described, but of rectangular shape, and the cross bracing being placed as the water was lowered; but as it would have projected beyond the fenders into the channel, and would almost certainly have been run into, it was deemed more prudent to sink a caisson with a coffer-dam top, which could be kept within the protection of the fenders. On account of the teredo, the caisson was planned so as to cut out the roof after the chamber had been filled with concrete, and the masonry started on the exposed concrete footing. To this end the caisson was built with a roof having a single layer of 12-inch timbers, with ceiling plank on the under side, the roof being broken into the spans with rods, as shown on drawing. The air chamber had a clearance of 7 feet. The bottom was prepared by dredging, when the caisson was floated into place and the coffer-dam sides were planked up and caulked as the caisson sank with the weight added from time to time, which, in addition to the water, consisted of the necessary quantity of ashlar stone, which would afterwards be built into the pier. After the caisson was landed on the bottom, sufficient stone was added to balance the air pressure and the concreting proceeded with in the usual way. When the chamber was filled, the coffer-dam was pumped out and stone removed, but the reduction of weight was too much for the concrete, which evidently had not bonded with the bottom, or possibly a leakage between the roof and concrete, or both combined, and the whole dam lifted up about 3 inches at one end. As it was hopeless to endeavor to force it back to place, and there was no telling how much the concrete was demoralized, there was no help but to take it out and begin all over again, which was done after a good deal of trouble. More care was exercised in removing the weight, and it was finally concluded to build immediately on top of the roof timbers and not undertake to cut them out. This simplified the work and the masonry was started in sections, until enough courses were built to insure

all the dead weight needed against the extreme head, when the rest of the pier was run up in the usual way. As the timber deck was below river bottom, a little rip-rap insured it against any possible exposure to the teredo. The pneumatic work and coffer-dam was performed under a sub-contract with Sooysmith Company.

Arthur Kill Bridge. Pier No. 5.

This pier is near the edge of the marsh forming the Staten Island shore, which is barely flooded at extreme high tides. Borings indicated about 30 feet from surface of the marsh to hard bottom, consisting of mud, mud and clay mixed, through more or less sand into clay, clay and shale to the bottom of shaley clay, on which the pier was to be founded. Experience on the Boston tanks seem to indicate that the founding of this pier would be accomplished with little difficulty. The area of the foundations was enclosed with a tongued and grooved sheet pile dam of 4-inch yellow pine plank; but it was found impossible to hold the plank at a depth of 15 feet; the mud and clay became puddled with water, and despite all efforts at bracing, the plank shoved inward to such an extent as to spoil the whole dam before we were half way down. A second dam was therefore driven around the first one, but this time with 10 x 12-inch tongued and grooved timbers, in one length, to reach to the extreme bottom. These timbers were grooved by slitting the grooves out at the mill with a circular saw and chiseling the blank so formed, free. The tongue was an independent "spline," $2\frac{1}{2}$ x 4 inches of dry wood and nailed in one groove. The timbers were shaped at the feet to drive close. This dam was hard driving, but was finally accomplished, when digging was resumed, and the old dam removed piecemeal as we could get in the braces. The bottom was reached within a perfect dam, with only one bad leak in the northwest corner due to the shattering of a small piece of one tongue during the driving. As it was impossible to stop this leak from the inside, and the outside was inaccessible, to prevent washing the concrete the leak was led off in a box at the side of the dam to the sump well, and the footing course of concrete, filling the whole area of the dam about 7 feet deep, was gotten in in place.